

179  
Vol. 46, No. 1

FEB 21 1949  
January, 1949

# Psychological Bulletin

EDITED BY

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WITH THE CO-OPERATION OF

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PUBLISHED BI-MONTHLY BY

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

1515 Massachusetts Ave., N.W., Washington 5, D.C.

Subscription price, \$7.00 per year, single issue, \$1.25.

Entered as second class mail matter at the post office at Washington, D.C., under the act of March 3, 1879. Additional entry at the post office at Menasha, Wisconsin. Acceptance for mailing at special rate of postage provided for in Section 538, act of February 23, 1925, authorized August 6, 1947.

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Psychological Bulletin, July, 1946

No. 4

85 pages

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American Psychological Association

1515 Massachusetts Avenue N.W.

Washington 5, D.C.

# Psychological Bulletin

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## INFANT CARE AND PERSONALITY<sup>1</sup>

HAROLD ORLANSKY

*Department of Anthropology, Yale University*

### INTRODUCTION

The importance of childhood experience to the formation of personality was one of the great findings of psychoanalysis. No social scientist will dispute that general finding, but all will wish to elucidate it.

The orthodox school of psychoanalysis, in keeping with Freud's genetic and biological orientation, has stressed the special significance of infantile suckling, excretory, and genital functions for the shaping of adult personality. Indeed, the Freudian oral-anal-genital characterology rests upon the assumption that specific channeling of these infantile physiological urges by varied parental care elicits specific psychological constellations in the individual.<sup>2</sup> The essential features of this characterology have been borrowed by some psychologists (116, pp. 362-385). Reasoning from Freudian theory, pediatricians and psychiatrists have advocated systems of infant care which they believe will promote the growth of secure and unneurotic personalities.<sup>3</sup> In general, these systems advise prolonged breast feeding on self-demand by the infant, gradual and late weaning, and the late and lenient

<sup>1</sup> The writer wishes to thank Drs. Louise Ames, Wendell Bennett, Ralph Linton, and Robert Sears for having been kind enough to read and criticize this paper in manuscript. If he had the ability and energy to utilize all their advice, it would be a better paper.

<sup>2</sup> For statements of this characterology, see Freud (43; 44; 46, pp. 135-140), Abraham (1, pp. 248-279, 370-417), Glover (54), Jones (77), Lorand (96), and Fenichel (36, pp. 62-69, 278-284, 487-496).

<sup>3</sup> A very extensive bibliography could be cited here which would be swollen by each succeeding issue of such journals as *Psychiatry* and the *American Journal of Orthopsychiatry*. Frank (38, 39, 40), Ribble (128), and Moloney (107, 108, 109) are among the ardent advocates of this program.

induction of bowel and bladder training. From the same theoretical base, some anthropologists and psychoanalysts have, in recent years, attempted to explain presumed character differences between members of different cultures in terms of their different patterns of infant discipline.<sup>4</sup>

Freudian theory, however, arose from clinical investigations of adult character, and the reconstructions of infant experience obtained from the analytic couch do not constitute empirical findings on the infantile situation. The work of child analysts like Melanie Klein and Anna Freud has added something to our direct knowledge of child psychology, but they have generally been more interested in applying psychoanalytic theory than in testing it. After a comprehensive review of the evidence, it was thus possible for three psychologists to say that "although we have now been exposed for some time to psychoanalytic and other psychiatric hypotheses regarding the effects of birth trauma, weaning trauma, extreme emphasis on early control of urination and defecation . . . we have almost no objective records of the development of children going through these experiences, or of experiments controlling certain aspects of the problem" (113, p. 575). Ten years later, Murphy opined that "some aspects" of these hypotheses had been experimentally confirmed, but "the observations are meager and the theory overpretentious" (114, p. 485).

Sears (136) has broadly surveyed objective studies designed to test psychoanalytic theory, but no one has yet concentrated attention upon the subject of infant disciplines and compiled the factual evidence on the relation between these disciplines and the development of personality. The present paper proposes to do this, gathering data from two sources: (a) empirical, experimental, and semi-experimental investigations in Western culture, and (b) evidence obtained from anthropological inquiries into other cultures. The writer is under no illusion as to the adequacy of much of the data which will be presented, and criticism will be offered where it seems appropriate. Nevertheless, he is of the opinion that the evidence permits definite, if negative, conclusions in regard to the effect of certain features of infant care upon personality; and where the evidence is less satisfactory, it is to be hoped that others will be encouraged to seek an empirical resolution of the problem.

<sup>4</sup> The writings of Gorer (58, 59), Erikson (30, 31, 32), LaBarre (64), and Roheim (134) illustrate the simplistic assertion that infant disciplines determine group character and culture. Their theoretical position should be distinguished from the efforts of such students as Kardiner (79, 80) and Du Bois (26, 27) to explain group character in terms of social and economic institutions and broad cultural patterns.



# FEATURES OF INFANT CARE

## *Nursing Experiences*

It has been argued that many features of the infant's early feeding experiences contribute to the formation of personality. Among the factors to which special psychological significance has been attributed are breast vs. artificial feeding, length of breast ~~feeding~~, self-demand vs. scheduled feeding, and weaning. Studies which have attempted to test the significance of these factors are summarized below, followed by a brief evaluation of their validity.

*Breast vs. bottle feeding.* The chairman of the Committee on Maternal and Child Feeding of the National Research Council concluded a recent investigation of the advisability of breast vs. artificial feeding with the following statement:

Although there is a voluminous literature on the subject of the emotional value of breast feeding to both mother and baby, it is regrettable that concrete evidence on this point is difficult to obtain. This is particularly regrettable in view of the rather emotional treatment the subject is receiving in both professional and lay circles. The necessary evidence will have to come from long observations on the development of the personalities of both breast-fed and artificially fed infants (3, p. 915).

While really adequate evidence is, to be sure, unavailable, the following studies have come closest to approximating the required observations.

Rogerson and Rogerson (131) traced 109 children who had been observed as infants in their London clinic seven years before. Those artificially fed during infancy had experienced more feeding difficulties, poorer health, and greater restlessness during infancy than had those breast fed during infancy. At the age of 7, the group which had been artificially fed in infancy was found to have a greater proportion of its members manifest poor appetite, poor sleep, enuresis, fears, nervousness, and a poorer showing in school than the breast-fed group. The authors conclude that "there is evidence strongly suggesting that there is a positive association between feeding difficulties in infancy and subsequent psychological difficulties at elementary school age" (p. 1181).

In a study at Joliet, Ill., Hoefler and Hardy (67) subjected 383 children 7-13 years old to physical and mental tests, correlating their ratings on the tests with the length of breast feeding in infancy. These findings will be reviewed below, but at this point we are interested in their observation that "Children who were artificially fed were, on the whole, inferior physically and mentally to the breast fed" (67, p. 619). The artificially fed children were more poorly nourished, more susceptible to diseases, and slower in learning to talk and to walk than were the breast-fed children.

Both the Rogerson and Rogerson and the Hoefer investigations were based on general populations, with no effort having been made to equate the groups studied for factors other than method of feeding in infancy. The physical and psychological difficulties which they found among artificially fed children can be accepted as descriptively accurate for their populations; but the inference that artificial feeding was a causative element in the creation of the difficulties is not warranted by these data. Indeed, Rogerson and Rogerson expressly disavow the propriety of inferring such a causal relationship (131, p. 1173). That ill-health and feeding difficulties in infancy can often be taken as evidence of emotional maladjustment is, of course, widely recognized.

Our inference that these difficulties were not necessarily due to the manner of infant feeding is supported by a study conducted by Faber and Sutton (34) which comes closer to the scientific ideal of controlled experiment than do the studies outlined above. These authors compared 42 babies at the Stanford University clinic who had been breast-fed less than six weeks and were thereafter reared on bottled milk with an equal number of babies who had been breast-fed for an average of forty weeks. In other respects than the type of milk, the diets of both groups were alike; 26 members of each group were paired for weight at birth, and all babies received "excellent care." It was found that the breast-fed babies gained more weight than the bottle-fed babies during the first quarter year, but this relationship was reversed during the rest of the year. The bottle-fed group had better health and fewer diseases than the breast-fed group. Accepting the psychosomatic criterion that physical and psychological health are apt to be correlated, this study would seem to show that artificial feeding need be in no way inferior to breast feeding in promoting the emotional well-being of the infant.

Summarizing the picture in the light of present data, it does not appear to be possible to establish preferential norms of breast or bottle feeding in our society without consideration of the class position of the parents and the psychological condition of the mother. The norm for lower-class families in America and England apparently entails breast feeding of children in a larger proportion of cases than does the norm for middle-class families;<sup>6</sup> different hospitals in the same city regularly have larger or smaller proportions of breast-fed babies in accordance (in part) with the class difference of their clientele (13). There is reason to believe that bottle feeding introduced into the average lower-class home might often have beneficial effects upon the child:

Babies between birth and about five months of age, when not breast-fed, . . . develop better in our nursery than in the average proletarian household.

<sup>6</sup> See Ericson (29), Rogerson and Rogerson (131, p. 1166), and Davis and Havighurst (18).

Their gain in weight is more regular and intestinal disturbances are less frequent. . . . In times of illness, the absence of tension and anxiety which the young mother invariably feels is certainly of advantage to the child.<sup>6</sup>

Above all, the character of the mother must be evaluated before an intelligent opinion can be ventured as to whether breast or artificial feeding will better serve to promote the healthy development of the child's personality:

In some cases, even though supplementary feeding may be necessary, breast feeding can advantageously be continued to foster the mother-child relationship. In other cases, it may be advisable, in order to minimize maternal rejection, to reduce breast feedings, despite the physical possibility of continuing them (48, p. 235).

We have seen no evidence to favor the current belief among some pediatricians that breast feeding is inevitably more advantageous to the child than artificial feeding.<sup>7</sup>

*Length of breast feeding.* In the study described above, Hoefler and Hardy found that the highest ratings in muscle tone, physical and mental development, and early age at walking and dentition were registered by children who, as infants, had been breast-fed 4-9 months; children breast-fed 3 months or less and 10 months or more had lower ratings than did this medial group (67).

Somewhat comparable results were obtained by Pearson in a study of 72 problem children at the Philadelphia Child Guidance Clinic. Children breast-fed less than 6 months or more than 10 months were more maladjusted at school and displayed timidity and nervousness with greater frequency than did those children who had been breast-fed from 6-10 months (117, p. 289).

Childers and Hamil (15) examined the records of 469 problem children at the Harper Hospital clinic in Detroit, and correlated the fre-

<sup>6</sup> Freud and Burlingham (42, p. 12). It is only fair to report that the authors add: "Breast-fed babies are, of course, better off than bottle-fed babies wherever they are. Our best results are found in babies who are breast-fed by their own mothers in our home" (p. 13). There are obvious reasons why a nursery baby who receives individual care from its mother should progress better than one who shares with many other babies the services of a professional nurse; but the assertion that breast-fed babies are invariably better off than bottle-fed babies remains unproven.

<sup>7</sup> Since breast-feeding is so frequently considered *de rigueur*, it may be well to note that a contrary opinion has been expressed by two outstanding pediatricians: "[Some people] . . . deplore the emotional losses which the bottle-fed baby suffers. It is probable that the nervous system of the modern infant is adaptive enough to meet most of the demands made by new modes of nutrition. Indeed, oncoming generations of infants by virtue of these very changes may even acquire emotional patterns of self-dependence which will ultimately add to the mental stamina of the race and form personalities better suited to the stresses of civilization" (Gesell and Ilg, 52, p. 42).

quency of occurrence of 95 "personality traits" with the duration of breast feeding in infancy. Children breast-fed 1-5 months showed a higher percentage of "abnormal behavior manifestations" than children breast-fed less than 1 month or more than 5 months. However, this study is quite unsatisfactory methodologically, and its results can be given little credence.

Maslow and Szilagyi-Kessler (102) gave a security-insecurity inventory to students in classes at Brooklyn College and correlated this measure of security with the length of infantile breast feeding. "The highest security scores were obtained by the groups who were breast-fed little or not at all and by those who were fed at the breast for over a year" (102, p. 84).

Peterson and Spano (118) correlated the length of breast feeding with personality ratings of nursery school children. They found "There is no evidence that those children breast fed least are more maladjusted and insecure than those breast fed longer"; and, again, "in the group of normal children studied . . . there is no relation between duration of breast feeding and child personality at the nursery school level, nor with adjustment at the pre-adolescent stage" (118, pp. 63, 65).

All five investigations, it should be noted, agree that there is no linear correlation to be observed between length of breast feeding and any major aspect of personality studied. This is the only definitive conclusion that present evidence seems to permit. On the basis of the Hoefer and Hardy, and Pearson studies one might speculate that a medial duration of breast feeding is more apt to be associated with "normal" personality development in our society than is either a brief or prolonged period of breast feeding, but this hypothesis is flatly contradicted by the Maslow and Szilagyi-Kessler finding. Barring questions as to the validity of the personality measures employed, it is clear that extreme caution is necessary in making any generalization applying to large populations from the very inadequate and unrepresentative samples upon which almost all of these studies were based. This is a conclusion to which we will have to return later.

The danger of such generalization can be illustrated by Levy's statement that "In general, all factors favoring rejection of the child tend to shorten, all factors favoring overprotection tend to lengthen, the breast-feeding act" (91, p. 59; cf. also 90, p. 101). Levy has probably contributed more to the experimental solution of the problems with which we are concerned than any other person, and his procedures are models in the field. The statement quoted above is based primarily upon research by Freeman (41) in which 100 long (12 months or over) and 100 short (1 month or less) breast-feeding cases were selected from a group of children at the New York Institute for Child Guidance, using no other criterion. It was then found that half of the children in the short-fed group were "rejected" and two-thirds of the children in the



long-fed group were "over-protected" by their mothers. There is no reason to dispute the accuracy of this finding or the reasonableness of Levy's generalization. But Peterson and Spano tested the generalization in their study of nursery school children, and concluded that "duration of breast feeding is not related to maternal rejection and cannot therefore be taken as an index of rejection" (118, p. 65). There may, of course, be some error in their procedure, although this is not apparent from a reading of the report. But is it just as likely that the groups studied differed in certain essential cultural characteristics, and that the contrasting generalizations advanced by Levy and Peterson are each valid only for populations which resemble the original groups in these (unexplored) characteristics.

*Self-demand vs. scheduled feeding.* The prevalent opinion in many circles is that self-demand feeding in which nursing always rewards the child's crying is better calculated to produce a confident and unneurotic personality than is rigidly scheduled feeding in which the clock rather than the infant sets the nursing time. The following quotations illustrate this viewpoint:

To the white child, whose feeding and other routines are rigidly scheduled, the mother or nurse . . . must appear incalculable. . . . He must develop a feeling that each individual is alone in life.

To the Navaho baby, on the other hand, other persons must appear warmer and more dependable, for every time he cries, something is done for him (85, p. 31; 82, p. 57).

The parent who responds to a clock rather than to the behavior of the child is, from the child's point of view, not responsive at all. . . . If the rewards which a child receives bear no consistent relation to his behavior . . . an apathetic or an anxious or a hostile individual is likely to result (110, p. 90).

Such statements must be regarded as hypotheses not facts; and, so far as the writer has been able to gather from a rather thorough search of the literature, except for two studies to be noted forthwith, they have never been subjected to any empirical test. A great deal of reading-in of adult emotion into the infant situation is evident in these interpretations. It is well known that only a few years ago the best "scientific" opinion favored scheduled feeding to promote healthy personality development in the child, and this view is still fairly widespread in the medical profession. Certainly, even very young infants learn to adjust their sleeping and eating habits to a schedule of regular feeding, and it would seem that this regularity might just as much serve to promote a feeling of security and confidence on the part of the child, as any other feeling. Here, too, we lean to the belief that a specific discipline does not exert a specific, invariant psychological influence upon the child, and that its effect can be gauged only from study of the parental atti-



tudes associated with its administration, of the child's constitutional endowment, and of the entire social-historical situation in which the emergent personality is located.

The two studies to which we have referred have not been fully reported, but the published account is such a choice specimen not only of difficulties of the particular problem with which we are concerned but of social science methodology in general, that we reproduce it nearly verbatim:

Miss Trainham . . . reported on a study begun at the Merrill-Palmer School. Two groups of children were chosen, one group on self-regulating feeding regimes, and the other not self-regulated. The parents of both groups were quite similar. . . . One group consisted of ten subjects and the other twelve. . . the results suggest in a very striking way that the self-regulated children are in all categories of the tests more advanced in their development. . . .

Dr. Escalona had conducted similar studies . . . and had arrived at opposite results. She, too, had only worked with small groups. One group consisted of babies on self-regulated feeding regimes, and the other group consisted of babies who were on the conventional rigid feeding schedules. The group of babies who were more frustrated according to the standards of this group [i.e., those on scheduled feeding] showed more rapid maturation by test scores. Until she had heard Miss Trainham's report, she had explained her findings by the theory that the frustrated babies are forced to recognize the environment earlier (109, p. 33).

\* Nothing is so easy to manufacture as a theory to "explain" a particular empirical finding. To explain *all* the relevant findings is not so easy, and we have no simple solution to offer in the present instance. Our means of measuring personality are not very adequate; our knowledge of its genesis is even less adequate.

But what *can* be asked of social scientists today is a minimum degree of historical and cultural sophistication which would enable them to recognize the position which the human groups that they study occupy in the *real* social world to which they—and the social scientists—belong (as contrasted to the position ascribed to them in the artificial world of the social scientist's "control" laboratory. Unfortunately, the social scientist cannot ignore the history of his subjects prior to the time that he has them in his laboratory, as the physical scientist can usually ignore the previous history of the materials with which he experiments). The scientist's observations would, then, at least have the validity of descriptive statements about a designated portion of the real world; ultimately, after a sufficient number of such statements had been accumulated, a fairly complete description of the real world might be obtained. To generalize about psychological or social laws of all humanity, however, on the basis of limited observations on small, unrepresentative groups, can only lead to such predicaments as the one described above.

*Weaning.* The writer has been unable to find any experimental data correlating different forms and times of weaning with aspects of personality, but a brief discussion of the question may nevertheless be in order. As an event which, in Western society, usually occurs within a relatively short space of time, weaning can be classified as a trauma. However, in keeping with his tendency to universalize Viennese cultural norms, Freud did not see the culturally relative nature of weaning, and conceived of it, instead, as an invariably painful situation for the child:

It looks . . . as if the desire of the child for its first form of nourishment is altogether insatiable, and as if it never got over the pain of losing the mother's breast. I should not be at all surprised if an analysis of a member of a primitive race, who must have sucked the mother's breast when he could already run and talk, brought the same complaint to light (46, p. 166).

This view that the infant has an "insatiable" yearning for the breast can only be regarded as an adult reading-in of emotion into the young infant. It is difficult to see why the purely physiological sucking capacity of the neonate can not be satisfied as well upon an artificial nipple as upon his mother's breast, or why a child who has never sucked his mother's breast need ever be weaned from it. Examination of the weaning trauma suggests that it may be regarded primarily as a product of a widespread (but not universal) social situation in which the child has come to associate not only nourishment but also warmth and affection with the mother's breast, and in which separation from the breast creates a fundamentally different social situation to which he must adjust.<sup>8</sup>

When weaning disrupts a series of satisfactions which the child has been receiving with regularity from infancy, it would be surprising if he did not react emotionally to this deprivation. One might suppose that late weaning would be more traumatic than early weaning, since in the former case the child would be more firmly accustomed to the satisfactions of breast feeding and hence more loath to forego them. Against this supposition the psychoanalyst counters that the developing child ego is better fortified to withstand the loss of the mother's breast at a later age; and that early weaning creates a craving for the oral satisfaction which has been denied,<sup>9</sup> resulting in a pessimistic or sadistic character, whereas late weaning results in self-assurance and optimism (36, p. 489). But this is all *ex cathedra* doctrine to the ag-

<sup>8</sup> Cf. Bernfeld: "In those children who receive their nourishment from a bottle given them by so many different people that they do not learn to recognize the mother (nurse) and source of milk, bottle, as a coherent whole loved object, the process [of weaning] is essentially changed" (11, p. 298).

<sup>9</sup> "One gets the impression that at developmental levels that do not afford enough satisfaction, the organism refuses to go further, demanding the withheld satisfactions" (Fenichel, 36, p. 65).

nostic,<sup>10</sup> in the absence of scientific documentation. A contrary hypothesis is that weaning, whether it comes early or late, gently or severely, can not be said either to weaken or strengthen the child's sense of security until the entire situation in which the act takes place and the changes it produces in the parent-child relationship have first been appraised. Under some circumstances, it is conceivable that severe early weaning may help to produce a strong, secure ego;<sup>11</sup> whereas, under other circumstances, the same weaning might contribute to the development of an insecure, anxiety-ridden personality.

*Thumb sucking.* As thumb sucking by infants and children has frequently been used as a diagnostic of oral deprivation in psychiatric and anthropological investigations, it is desirable to appraise the validity of this criterion.

Levy (87, 89) has experimentally confirmed the young animal's drive to exercise its sucking function in various ways when this had not been adequately exercised in the normal process of nursing, and he has demonstrated that thumb-sucking in the human infant is *often* a response to sucking deprivation (86, 87, p. 203). Roberts (130) has supported the latter finding.<sup>12</sup> Surveying these data, Sears concluded that

On the whole, evidence supports Freud's view . . . that thumbsucking is a reaction to deprivation. It occurs not uncommonly when the nipple is withdrawn from an infant's mouth and its chief antecedent condition, as a habitual activity, is inadequacy of opportunity to suck extensively in connection with the eating process (136, p. 4).

There is absolutely no question that thumb sucking *may be* "a reaction to [oral] deprivation." But is important to point out that (1) oral deprivation *need not* result in habitual thumb sucking<sup>13</sup> and (2) when thumb sucking does occur, it is *not always* attributable to oral depriva-

<sup>10</sup> Some of the more devout have also expressed the opinion that the oral character is not clearly defined. See Fenichel (36, p. 488).

<sup>11</sup> Cf. Bernfeld: "The early, sudden, frustration . . . would offer the possibility . . . for the unfolding of an energetic ego" (11, p. 299). The logic behind this argument is that weaning serves to separate the mother-image which is originally merged with the child's ego, thus encouraging an early independent development of the child's ego. Bernfeld elaborates this view into a general hypothesis that peoples who wean their children early have a more well-defined sense of reality than do peoples who wean their children late (p. 294).

<sup>12</sup> In this study of 15 thumb suckers and 15 non-thumb suckers 7-8 months old, she observed that "non thumb suckers took a longer time for feeding than was taken by thumb suckers" (130, p. 7). Cf. also Ericson (29, p. 499) and Davis and Havighurst (18, p. 707) who note that proportionately three times as many middle-class as lower-class children, in Chicago studies, were thumb suckers; this can be correlated with the lesser opportunities which the middle-class child was afforded for sucking while nursing.

<sup>13</sup> Levy notes that "the submissive child is more likely to yield a finger sucking pleasure than an aggressive one, regardless of the quantity of oral tension" (87, p. 206).

tion. As the latter point has not received the attention it deserves, some documentation for it may be offered.

Thumb sucking has been observed at normal delivery (76, p. 84), and Bernfeld reports that it is present in *all* infants during the first three months.<sup>14</sup> Thumb sucking has been noted for a minority of children in primitive societies where unlimited nursing on self-demand is the rule.<sup>15</sup> An interesting case in our own society concerns twins raised on a self-demand schedule:

Each infant was allowed to nurse at any time as long as desired, and nursing periods ranged from three minutes to fifty-five. Sherry . . . never sucked her thumb while Peter sucked his almost constantly at four months and in moments of stress at thirteen months (141, p. 176).

In a Chicago study, Davis and Havighurst found that

Negro middle-class children are treated much more permissively than the white middle-class children with respect to feeding and weaning. . . . Yet the proportion of Negro middle-class children reported sucking their thumb is almost the same as the proportion of white middle-class children so reported (18, p. 707).

Gesell and Ilg (52, p. 124), and Bakwin (6) have voiced their conviction that oral deprivation is a factor "of minor importance" in the general etiology of thumb sucking. The latter writes, "We have frequently seen babies nursed at the breast and seemingly satisfied in every way, who sucked their thumbs; and we have seen these babies develop later as well-adjusted children" (6, p. 100).

An adequate statement of the relation between thumb sucking and oral deprivation, thus, appears, to be: (1) thumb sucking is often the

<sup>14</sup> He writes: "... all doubt concerning the regularly-established fact that all children suck their fingers even without being hungry is absolutely excluded during the first three months . . ." (11, p. 73).

<sup>15</sup> Mead's unqualified statement that "no primitive child whom I have ever seen or heard of sucks its thumb or fingers," which appears in an authoritative handbook of child psychology (104, p. 676) and which was repeated at the May 29, 1948 meeting of the Society for Applied Anthropology in Philadelphia, can only be deplored as an inaccuracy. Dennis has noted that of 288 Hopi children 2-9 years old, 13 or 6% were definite cases of thumb sucking (22, p. 181); Ward Goodenough informs me that cases of thumb sucking are to be observed among children on the island of Truk, despite unlimited breast feeding throughout the first year; and similar data could no doubt be obtained for many other tribes if sufficiently accurate observations were available. The more moderate thesis that finger sucking is comparatively rare among many peoples with adequate breast feeding of children remains, of course, in line with the evidence. Thus Kluckhohn notes that among the Navaho "Thumb-sucking is very rare, and we have never observed 'obsessive' thumb-sucking or a child asleep with its thumb in its mouth. When excited or disturbed, a child will not infrequently put the index or some other finger in its mouth" (82, p. 54).



result of previous sucking deprivation (the child desiring to gain a pleasure which he has been denied); (2) it may also result from *excessive* gratifications (the child desiring to perpetuate or reconstitute a pleasure which has been gratified); (3) it may be associated with neither deficient nor excessive, but with entirely normal sucking experience. That thumb sucking may also result from factors extraneous to oral gratification is not to be excluded. It is clear, in any case, that no single-factor explanation of its etiology will suffice, and that it cannot, therefore, be used as an *unqualified* index of oral deprivation.

### "Mothering"

In a series of widely quoted publications, Ribble has waxed rhapsodic about the importance of adequate "mothering" to the development of sound personality and organic health. By "mothering" she means the tactile contact, cuddling, and stimulation of the infant "by one individual" (127, p. 459). Such mothering, she contends, "primes the breathing mechanisms," improves "the tone of the gastro-intestinal tract," "fosters functional integration," relieves "muscular tension," strengthens the infant's "sense of equilibrium," and gives him "a sense of security" (127, 128, 129). The sound of the mother's voice is also held to contribute to the physical and emotional well-being of the infant: "singing to the young infant soft, simple, rhythmical songs brings a sense of relief and security to his sensitive nervous system and feeds his stimulus hunger" (128, p. 45).<sup>16</sup> Finally, the infant is said to have a physiological need for sucking which helps organize his respiration, blood circulation, digestion, the development of facial muscles and of the brain, coordination of the eyes, "the age at which speech appears," etc. (125, 126, 128, 129).

It is unfortunate that such an influential writer has not attempted to draw a line between her empirical findings and her personal opinions. There is so much panegyric and so little satisfactory evidence in her writing that it is difficult for an impartial critic to evaluate many of her statements objectively. One gets the general impression that every infant hovers on the border of death, and that the balance in the direction of life must be tipped by adequate "mothering" and sucking. After a while, one wonders how it is that babies who are not fondled and sung to ever manage to survive at all.

That the sucking mechanism of the neonate is developed at birth is, of course, well known; and, in the sense that most mechanisms are made to be used, it may perhaps be said that a "drive" to exercise its sucking

<sup>16</sup> Cf. Bernfeld: "Compaire emphasized strongly the significance of the mother's voice for the child's development; indeed, he becomes almost pious when he speaks of it" (11, p. 88).



function exists at birth independent of the infant's need for nutriment. Ribble asserts that this drive must be encouraged and satisfied if the infant is to survive and prosper, stating that "Fifty per cent of the 600 babies in our . . . study were definitely not 'self-starters' in sucking" (129, p. 628). Other authorities, however, have stated that "Sucking is present in all infants after 24 hours" (137, p. 64); and there can be little doubt that spontaneous thumbsucking, which is all but universal among infants, can serve to satisfy whatever physiological "need" may exist for exercise of the sucking function, where this is not satisfied in the normal process of nursing.<sup>17</sup> (See the previous section on thumb sucking.)

It seems to us, therefore, that Ribble takes too hysterical a view of the neonate's organic and psychic resources, which are considerably greater than she allows and which enable it to thrive under a much wider range of care than the modern American pediatrician advocates to the American middle-class mother.<sup>18</sup> The same point can be made in regard to the supposed need for "mothering" during the first few months of life. The human infant is obviously incapable of surviving without adult care and protection and, if one wishes, one may thus speak of his organic "need" for such care. But the minimum needs thus defined are probably much less than the pediatrician is apt to prescribe.

The pediatrician's prescriptions are, in general, designed to recreate the sheltered position which the fetus occupies in the womb.<sup>19</sup> We do not

<sup>17</sup> We are, however, by no means convinced that the activity of sucking is physiologically necessary for infant health, but only that it is the normal complement to it: i.e., we believe that infants restrained from thumb sucking and fed intravenously could be potentially as healthy as normally suckled infants, and would probably even lose their sucking urge, just as it has been found that "if chickens are fed from a dropper for a long period of time after birth, they lose the pecking urge" (Levy, 92, p. 659). A study by Sears on cup feeding of infants seems to support this view: "In general, the results indicate that breast feeding produces an oral drive, or at least strengthens it, and cup-fed children do not have as much oral activity during the first ten days of life as breast fed children." (Personal communication. A report of this study is in press in *Pediatrics*.)

<sup>18</sup> There are, of course, pediatricians of a broader outlook who are aware of the fact that infants in other times and places have managed to grow to healthy maturity without having enjoyed the care and advice of pediatricians. Gesell and Ilg are among these: "Fortunately the factors of developmental safety are so great that most children weather the everyday crudity of our methods of management" (52, p. 131).

<sup>19</sup> "The manifold prescriptions which science gives the mother for the care of the new-born can be summarized in one phrase: warmth and protection from stimuli. This means that our care of the infant attempts to reconstruct and strengthen the psychophysical condition in which we found the foetus" (Bernfeld, 11, p. 3). "... natural intuition ran ahead of science, and with swaddling clothes, and the cradle, which was to imitate the bobbing about in the amniotic fluid, created an artificial substitute for the uterus which had been abandoned" (Alexander, 4, pp. 123-124).

wish to imply that this treatment of the infant is *not* good for it. But as Bernfeld has observed, there are many cultures in which infants are regularly subjected to treatment which, in almost every particular, violates the "foetusphile" prescriptions of the pediatrician, and in which they still grow up into normal, healthy adults:

If we combine these details into a composite description, we gain a conception of child rearing which represents the exact opposite of the foetus situation. The child . . . is bathed or washed in cold water; it is placed on a flat pad without coverings, or is swaddled or left to lie motionless (11, pp. 6-7).

Indeed, Bernfeld feels obliged to coin the term "foetusphobic" to describe the type of infant care prevalent in so many societies. There are cultures where "no attention is paid to the filth which accumulates and . . . the babies are caked with dirt" (37, p. 83), where the infant's head or feet are deformed, or where its nose, ears, or mouth, are subject to piercing and its body to scarification or other painful mutilation.<sup>20</sup> Until contrary evidence is forthcoming, there is no reason to believe that infants reared in a "foetusphobic" manner are any more neurotic or insecure, or differ in any significant aspect of personality from the "foetusphile" infants *because of any single type of treatment to which they are subjected*.<sup>21</sup> That foetusphobic upbringing will result in the death of a high proportion of infants is quite another matter; it may also ensure that only strong and healthy infants survive to maturity, whereas

<sup>20</sup> Ploss (121, Vol. 1, pp. 288-338) gives an extended account of such treatment to which many primitive infants are subjected. The typical Flathead adult, e.g., had a head which was so flattened that the top of the skull, viewed in profile, was only one and a half to two inches wide. Mason expresses the belief that such deformation did not cause much pain "as it is done in earliest infancy, while the bones are soft and easily depressed" (103, p. 173).

<sup>21</sup> Greenacre has declared that "heightening of the sado-masochistic elements of the character . . . would seem to be the inevitable result" of bodily deformation, mutilation, or restraint in childhood (61, pp. 216-217). Such statements are based upon psychoanalytic principles derived from observations in Western society where mutilation carries sado-masochistic implications; that they reveal a lack of insight into the nature of culturally conditioned behavior is indicated by her reference to "the underlying sadistic aggression from which such frightful practices of restraint originate" (p. 216). The anthropologist must observe that the primitive parent who deforms the head of his child is not necessarily being any more sadistic than the Jewish parent who has his child circumcized or the American mother who lets her daughter's hair grow long and combs it daily, to the child's extreme discomfort; each parent merely conforms to culturally defined norms of religious behavior or of beauty. And if the anthropologist is led to protest against psychological explanations of the origin of cultural phenomena (see White, 147), surely he may also protest against psychoanalytic interpretations of personality which are based upon cultural facts whose meaning has been misconstrued.

foetusphile practices encourage the preservation of infants with weaker constitutions.<sup>22</sup>

Our argument has been directed against an exaggerated insistence upon the need for tactile stimulation of the infant by the mother for the maintenance of its mental and bodily health. We do not mean to suggest that *some* contact and, more important, affection from the mother or her surrogate is not necessary if the infant is to grow into a "healthy" individual (i.e., one conforming to an ideal-typical norm held by Western psychiatrists). Spitz has shown that when nursery children six to eight months old have their mothers removed, some of the children develop depressive symptoms, which he attributes to their loss of love object (138). Ribble describes negativistic or depressive states arising in children deprived of their mothers which, apparently, have a similar etiology.<sup>23</sup> The high death rate in foundling homes, despite satisfactory hygienic conditions, has been cited as evidence of the child's need for attention and love if it is to survive; however, it is unclear whether love or individualized care is the crucial desideratum here (any uniform institutional regime which is not adjusted to the different constitutions of its members is apt to lose those members whose constitutions can not adjust to it).

A crucial experiment conducted by Dennis (19, 20) throws some light on the question of how much and what kind of attention the infant requires if its physical and emotional development is to proceed unimpaired. He raised twin girls from birth in conditions of extreme isolation. The infants were separated from each other by a screen and received only the purely minimum attention required for feeding and bodily care, being kept supine on their backs and not spoken to or fondled or played with or exercised in any way until they were seven months old. Because of their restricted practice, visually directed reaching and grasping, sitting alone and standing with help were retarded, but otherwise, the children "yielded during most of the first year a record of development not distinguishable from comparison records of infants in normal environments" (20, p. 157). The children responded affectionately to the experimenter during his brief visits, so that Dennis remarks: "Fondling is not necessary for the development of interest in, and every sign of affection for, the adult" (20, p. 156). His over-all conclusions agree with the ones we have drawn:

<sup>22</sup> Bernfeld has suggested that the foetusphile practices of modern cultures owe their origin "primarily to the economic-political interest of the nations in large populations" (11, pp. 7-8).

<sup>23</sup> Ribble (127, p. 461; 129, p. 633). Cf. Levy (88).

... normal behavioral development can occur in some infants when most of the first year is spent under conditions of minimum social stimulation and of very restricted practice. In other words, a large number of acts on the part of the adult which have been held by some people to be of importance may be dispensed with (20, p. 156).

Similar conclusions can be drawn from the investigation of two Viennese psychologists who compared the development of Albanian children raised under conditions of great physical restraint, with that of Viennese children (17). Until they are one year old, the Albanian children are bound securely to a wooden cradle customarily placed in the darkest corner of the room, often with a cloth thrown over their heads so that no light is visible. These children displayed poor muscular coordination, but once given an opportunity to practice, their performance improved rapidly so that it was clear no permanent retardation had been effected. Their social behavior, as measured by responses to the experimenters in a series of standardized tests, was equal or superior to the norms for Viennese children of the same age.<sup>24</sup> Identical observations could, undoubtedly, be made on the children of many primitive peoples who, tied securely to cradleboards during their first year of life, may experience comparatively little bodily contact or fondling by the mother.<sup>25</sup>

An important consideration to be kept in mind in evaluating the relation between the type of physical care which a child receives and its emotional development is the constancy with which the care is administered. It should be noted that in the cases of anaclitic depression reported by Spitz the children affected had each been accustomed since birth to the care and attention of their mothers. One wonders if the same reaction would have occurred had they (like Dennis' experimental twins) not been accustomed to such personal attention during their first half year of life.

Another point which would bear investigation is the extent to which

<sup>24</sup> Mead comments on this Albanian study that "we do not yet have any way of measuring the change in quality which is introduced when an activity is engaged in later than was organically possible" (106, p. 72), her inference being that some change in quality *has* been introduced. However, by operational rule, it is meaningless to talk of a change until it is possible to measure that change. Elsewhere, she suggests that children who have been bound to the cradle "will differ in manner of walking, occasions when they walk, significance of walking for the personality, type of balance disturbance to which the individual is subject, etc." (105, p. 673). These are more measurable indices, but they have not yet been demonstrated.

<sup>25</sup> A number of other peoples who subject their children to extreme restraint during the course of infancy are mentioned in the following section on restraint of motion.



care by a *single* adult is requisite for the development of "normal" personality.<sup>26</sup> Ribble, it will be recalled, makes this assertion but the anthropologist will suspect it may be predicated upon the monogamous, nuclear family system of Western society. Need the child who grows up in a society with a strong, extended family maintain only one love object during its formative years? Such a child cared for by siblings and aunts would hardly be expected to develop the abnormally withdrawn personality common to so many orphaned and unloved children in our own society. In short, Ribble and those who share her beliefs are not so much making an absolute judgment on the type of care which is necessary for sound personality formation as they are making a series of recommendations which, implicitly, are based upon the nature of the child's social environment in the Western family and upon the nature of the family's position in Western society. Scientific investigations of desirable patterns of child rearing might proceed more successfully if the investigators were more conscious of this fact.

### *Sphincter Training*

The anal-erotic, anal-sadistic or compulsive character is the most clearly-drawn picture in Freud's album of characterology. According to this picture, the infant derives a great deal of erogenous pleasure from its bowel movements, and when this pleasure is inhibited by severe bowel training,

The usual result is reaction formation . . . in the form of a drive to limit all his primitive, impulsive, pleasure-seeking activities. . . . In extreme cases . . . one becomes parsimonious, stingy, meticulous, punctual, tied down with petty self-restraints. Everything that is free, uncontrolled, spontaneous is dangerous . . . (114, p. 747).

So widespread is the anal character in Western society that Roheim has argued that the entire culture "is based on sublimations or reaction formations of anal trends" (132, p. 283). Elaborating the Freudian theory, some psychiatrists have formulated a system which directly correlates adult character traits with specific forms of sphincter training in childhood:

. . . it appears that the child who has been trained in continence of urine and faeces slowly and without pressure or punishment will yield control over his eliminations without anxiety or resentment, learning to release without con-

<sup>26</sup> Studies of personality development in orphanages and public institutions which Freud and Burlingham (42) and Goldfarb (55), among others, have conducted, throw some light on this question, but the abnormal nature of this environment in our society requires that generalizations from their findings be made with caution.



flict; but a precocious or harshly coercive training that forces the child, before physiologically ready, to release to the outside demands, will set up resistance, accentuate retention as a defensive response and focus the child's behavior upon acquisitive or compensatory outlets for the denial of possession of his own eliminations (39, p. 22).

In line with this theory, Gorer (58, 59) has attributed many features of Japanese culture and character to the rigid bowel training of Japanese infants.

However, when one comes to look for empirical verification of the influence of sphincter training upon personality development, evidence which would compare the personality traits of two groups of children whose home environment was similar except for the time and stringency with which sphincter training was imposed, the data are too meagre either to substantiate or to disprove the Freudian thesis. It is unfortunate that so little attention has been paid to this particular problem, since it appears to be the area of infant care in which experimental disposition of psychoanalytic precepts could most readily be made.

Huschka (71) studied the reaction of 213 problem children at a New York hospital to the imposition of bowel training. She considered as "coercive" training which was started before 8 months or completed before 18 months, whereas bowel training initiated after 8 months and completed after 18 months was considered "adequate." She found that children who were "coercively" trained manifested "undesirable" responses such as constipation, fear, and rage more frequently than did children who were "adequately" trained; but there was no follow-up to correlate these responses with any permanent effects upon personality, and no control of normal children was used for comparison with the problem children. In a similar study, the same author secured data on the reaction of 215 problem children to the institution of bladder training (72).

Hamilton compared a group of 200 adults who recalled constipation or other anal-erotic traits in childhood with a group who did not in personality traits of stinginess, sadism, masochism, and concern for clothes; he found some evidence in favor of Freud's theory of the anal origin of these traits (63, pp. 467-472).

These fragmentary and inadequate studies on sphincter training and personality are all that we have been able to discover in the psychological literature. Ardent believers in psychoanalysis have shown a tendency to regard these and even less substantial data as confirmatory of their hypotheses, but a reading of the original studies does not incline an impartial critic to such optimism. Thus, one recent writer declares that

"Despert found that early training produced an over-organized and compulsive personality" (98, p. 411). Reference to the original paper by Despert (24), however, reveals nothing to confirm this judgment, which can only be interpreted as a wishful reading-in of conclusions unwarranted by explicit data. The same writer, reviewing a series of studies by Fries, states that

... the role of habit-training was studied thoroughly. That it influenced behavior was proved beyond doubt. ... It can be assumed that early training is very likely to have ill effects on emotional development. ... Untoward results are to be expected. What these results are in specific behavior has not yet been tabulated and set in conclusive terms. ... Nevertheless, it is conceivable that eventually more specific cause-effect data may be forthcoming (98, p. 410).

Fries herself, however, subsequently drew a contrary inference from her work, stating that "any attempt to correlate age at which habit training was started with later personality traits, without consideration of the mother's personality type, would involve a large factor of error (49, p. 92).

In the light of the emotional argument in many psychiatric circles about the contribution of early sphincter training to the development of neurosis, a few words of caution as to the validity of this thesis may well be offered. Only fifteen years ago, an authority (149) writing in the standard handbook of child psychology advised that bowel training be started as early as six weeks, while another child psychologist stated that bladder control might profitably be initiated at 3 months (135, p. 74). The shift in opinion that has taken place in the past decade must be regarded as due to a middle-class fashion of the times rather than to any conclusive scientific validation of the preferability of later sphincter training. Against the swings of custom, of course, rational argument is futile.

A common reason which is advanced in support of the desirability of late bowel training is that myelinization of the pyramidal tract is not completed until the child is at least a year old, and therefore earlier habit training makes demands upon the child which its organism is not yet mature enough to fulfill voluntarily; i.e., the child "has to rely on the local conditioning of the rectal sphincter by reflex action rather than on voluntary control" (71, p. 305). The relevant neurological facts are as yet, however, inadequately known:

... much is still to be learned about even the neurological aspect of the optimum age at which to begin bladder training ... it is not yet possible to make any definite statement about the time at which myelinization becomes complete. ... [F. R. Ford] concludes that the sequence in which the function of various structures develops undoubtedly corresponds to the sequence in which

they become myelinated, but . . . some degree of function may appear in the absence of myelin. Also . . . Langworthy has suggested that myelinization may be the result of development of function rather than the cause (72, p. 256).

In opposition to the orthodox Freudian's emphasis upon strict bowel training as the causal agent of anal character formation, Horney has advanced a view we are inclined to accept which explains anal character traits as arising from a total social situation:

. . . instead of relating these traits to the . . . "anal" sphere, one would understand them as a response to the sum total of experiences in the early environment. Thus the difference in point of view may be expressed . . . a person does not have tight lips because of the tenseness of his sphincter, but both are tight because his character trends toward one goal—to hold on to what he has and never to give away anything (69, p. 61).

Kardiner has pointed out that in a society which demands early bowel training of its children, "cleanliness and orderliness are forms of acquiescence to cultural demands, and are therefore forms of obedience" (79, pp. 43-44). Obedience, punctuality, cleanliness, hoarding, and the other "anal" character traits are certainly inculcated into the child by middle-class parents of capitalist society, this syndrome of traits being in part historically associated with the Protestant virtues which were so well rewarded during the rise of capitalism, and having also an independent culture-history of their own.<sup>27</sup> In these terms, the origin of the anal character can be understood as an adjustment of the individual on the level of personality to concrete economic and cultural conditions. The contrary contention advanced by Roheim, that these conditions are due to severe anal training, is historically untenable: a person can hardly hoard money, e.g., or be compulsively punctual if there is no money to hoard and no clocks by which to measure punctuality; and need it be added that money and clocks serve rational purposes and that their development can be explained historically without reference to the infant libido?

Outstanding among psychoanalysts in his recognition of the impor-

<sup>27</sup> See Weber (144). Kroeber (83, pp. 600-602) furnishes a brief culture-historical sketch of the distribution of traits of cleanliness and order in time and space. He points out that the early Protestants, rebelling against the conservative and clean clergy of the 1400's, "washed little and looked on bathing as rarely needed and as likely to be dangerous to health. . . . This phase passed away with the slackening of religious intransigence, with the growth of enlightenment, with the increase and spread of wealth and therewith of comforts, with the rise of the bourgeoisie. . . . It was the countries in which the new wealthy middle class became specially influential that took the lead in the new direction; hence the scrubbing of Dutch doorsteps and the proverbial Englishman with his portable bath" (p. 601).

tance of social conditions in the channelling of libidinous forces, Fenichel (35) has argued persuasively against Roheim's position on just this point. But even Fenichel holds that "anal eroticism produces the desire to collect something," though adding that "What is collected is determined by reality" (p. 89). To speak of a general instinctual "desire to collect something" as existing divorced from the material demands of a specific cultural situation is, however, to invoke gratuitously a factor for which independent evidence can in no case be given. It is true, to be sure, that certain animals (e.g., rodents) hoard, but as the human animal does not exist separate from his culture, it is fruitless (untestable) to speculate about what might be his drives in a culture-less circumstance. The more economical theory would appear to be to explain "anal" personality traits in terms of the cultural universe which gives rise to them.

### *Restraint of Motion*

The problem of the child's emotional reaction to physical restraint has received a good deal of attention in the anthropological and psychiatric literature. Dennis (22), in particular, has advanced what appears to us to be an excellent solution of the problem, and his paper can be consulted by those who wish a fuller documentation of our position. Greenacre (61) and Levy (92) have also contributed summaries of the literature.

In the course of a report published in 1917 which described various experiments they had performed on newborn infants, Watson and Morgan declared: "If the face or head is held crying results, quickly followed by screaming. . . . Almost any child from birth can be thrown into a rage if its arms are held tightly to its sides" (143, pp. 166-167). This statement was repeated by Watson in many publications which appeared during the following eleven years.<sup>28</sup> It gained credence from an interpretation of the Freudian frustration-aggression thesis advanced by some scientists as a universal principle of human psychology: that aggression must always result from frustration.<sup>29</sup> Neither Watson's original finding nor the Freudian thesis, however, has (we believe) been validated by empirical investigations.

A number of psychologists tested Watson's observation in experiments on newborn infants. Taylor found that in response to restraint of the arms no reaction occurred frequently enough to be called typical,

<sup>28</sup> A list of these publications, appearing from 1917-28, is furnished by Dennis (22, pp. 202-203).

<sup>29</sup> "... the existence of frustration always leads to some form of aggression" (25, p. 1).

*frustr. → aggr.*  
*not*  
*aggr. always results from frustr.*



and only 32 per cent of the infants tested made *any* response described by Watson as a reaction to restraint (140, pp. 79-80). Pratt, Nelson, and Sun found that in 58 per cent of the cases tested, infants remained passive in response to restraint of their arms: "no definite 'defense' or 'rage' reactions were found in any considerable number of cases" (123, p. 182). Stirnimann studied the reactions of 150 newborn babies to the tying of mittens on their hands. Twenty-three babies got rid of the mittens; but most "showed no reaction unless the gloves were applied just before meal time."<sup>30</sup>

Prolonged restraint of infants is common and often very severe in cultures where it is the custom to swaddle the newborn child or tie it to a cradleboard during the first year of life. Among the Comanche, for instance, it is stated that "Children of six months when taken off the cradleboards had arms as stiff as though they had been pinioned" (95, p. 73). The Chinook child's head is bound to a cradle immediately after birth and the binding is not removed for a period of anywhere from three months to a year (103, p. 176). Throughout the first year, Albanian children are released from their cradles only once a day for a brief period of cleaning (17). The custom of infant restraint is very widespread:

In both Americas the majority of aboriginal children were confined in some sort of cradle from their birth until they were able to walk about (103, p. 161).<sup>31</sup>

... swaddling clothes were employed by the Jews, the Greeks and the Romans... and were used throughout the medieval period in all European countries. Swaddling did not begin to disappear in England until 1750 (22, p. 209).

Reports vary on the initial reaction of infants to the imposition of restraint in the form of cradling or swaddling. Often the child cries, kicks, or otherwise protests;<sup>32</sup> at other times the swaddling process evokes no complaint.<sup>33</sup> But it seems to be the unanimous testimony of

<sup>30</sup> We have been unable to consult the original reference (*Revue Fr. du Pédiatrie*, 1937, 13, 496-502) and so this account is taken from Levy (92, p. 660).

<sup>31</sup> According to Pettitt, the cradleboard was used in every cultural area in North America north of Mexico, except for the Eskimo and a few tribes of the Southeastern United States; and, with few exceptions, by every tribe within these areas (119, p. 12).

<sup>32</sup> Linton reports that Comanche children "always kicked and cried when they were being wrapped" (94, p. 472); the Albanian child at least occasionally cried when strapped into the cradle (17, p. 229).

<sup>33</sup> "Winifred de Kok... describes the swaddling of Italian children as an entirely happy proceeding..." (61, p. 212).

"The unanimous testimony of Hopi mothers affirms that when the infant is tied to the cradleboard on the first day of life the process does not cause the baby to cry" (21, p. 95).



observers that once the child has been placed in restraint, it accepts this restraint without complaint during the first few months of life. (It is difficult to imagine what else the child *could* do but submit, when it is securely bound and crying is not rewarded by any loosening of its bonds.) In fact, when removed from their cradles during the first six months, Navaho and Hopi children often cry to be put back in them.<sup>34</sup>

As the child grows older and as it experiences increasing periods of freedom from restraint in the course of cleansing, play or exercise, it often will protest vociferously against the application of restraint.<sup>35</sup> On the basis of these data, Dennis has come to the following conclusions on the child's reactions to restraint, with which we are in agreement:

[The young infant] . . . has no unique pattern of reaction to restraint. He reacts to all strong and persistent stimuli with restlessness and crying. . . . On the other hand, methods which achieve restraint of movement without steep gradients of pressure . . . do not produce restlessness and crying. . . . Whether or not restraint of movement will cause negative reactions in the older infant depends upon whether or not the restraint interferes with customary sequences which have been built up. *What is thwarting to the infant is not predetermined but depends upon post-natal events* (22, pp. 211-212, 216). [Our italics.]

In short, children who are accustomed to restraint do not experience it as a frustration; but once accustomed to freedom, the same restraint may become a frustrating experience. If they have never experienced restraint its imposition would, of course, generally be expected to provoke a hostile reaction.<sup>36</sup> These are substantially the same conclusions to which Levy was led;<sup>37</sup> and he also adds the important consideration

<sup>34</sup> "Young [Navaho] babies will often cry to be put back into their cradles, and many of them do not sleep satisfactorily anywhere else" (85, p. 26).

"When the Hopi infant who has become accustomed to the cradleboard is kept off the board for a longer period than is usual he cries and does not go to sleep" (21, p. 96).

<sup>35</sup> "After about the sixth month . . . the [Navaho] infant apparently begins to feel the confinement a frustration and will wail to be released" (85, p. 26).

"Some Pueblo Indian infants at several months of age begin to object to being placed on the cradleboard. But it is important to note that the Indian infant of this age has been off the cradleboard each day for an interval sufficient to accustom him to manual play and to other activities" (22, p. 213).

<sup>36</sup> In this connection, Dennis reports that one Hopi mother who raised her daughter in freedom later tried to strap her to the cradleboard at five months, but the child struggled so strenuously that the mother finally relinquished the attempt, a "general experience of mothers who have given birth to a child away from the reservation, where cradleboards are not used, and who have returned to their native village when the infant was several months of age" (21, p. 98).

<sup>37</sup> " . . . after a child has experienced creeping or walking, a restriction of these activities will be felt as restraint. The modification of activities before they have been fulfilled may be easily achieved. . . . Once experience of free activity has taken place, the situation is quite different" (92, p. 659).

that constitutional differences in the need of movement will influence the child's reaction to restraint.<sup>38</sup> These considerations must be known to psychiatrists who argue that restraint of motion is inevitably frustrating, for some of the same psychiatrists use restraint to effect the sedation of their over-excited mental patients. There appears, then, to be no uniform emotional reaction on the part of the child to a uniform type of restraint, but rather various reactions which differ primarily in accordance with individual constitution and previous experience.

*Infant "frustration" and "aggression"*

It appears from the discussion of restraint (and, we can surmise, what is true of infant reactions to restraint may also to some extent be true of responses to other disciplines as well) that an experience which psychologists often designate as "frustrating" may in reality not be frustrating at all to the infant. Two outstanding factors enter into the explanation of those cases in which infant restraint was not experienced as frustrating: (1) the previous experience of the infant conditions it to accept restraint as normal, and (2) restraint is only part of a total situation in which the child finds itself and which contains many positive rewards (counterbalancing, apparently, whatever negative features may also be present). Thus, for example, the restrained Navaho child is protected "from the painful outcome of motor activity, such as picking up a hot coal or getting in the way of adults" and, at the same time, may actually participate in the social life of the family at an earlier age than does the uncradled American child.<sup>39</sup> Contrariwise, there is no society,

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Cf. Hartmann, Kris and Loewenstein: "early swaddling . . . does not stimulate aggressive response, since it does not interrupt an activity but prevents one" (65, p. 25). These authors, however, also repeat the unjustified assertion that "By and large, the child tends to react to restraint by some manifestation of aggression" (*ibid.*).

<sup>38</sup> " . . . the need of movement varies according to . . . innate differences, maturation, health, type of restraint, and emotional factors" (92, p. 662).

Fries also lends support to the importance of constitutional factors in determining the child's reaction to restraint. "Children differ in their Congenital Activity Type. The difference in responses of the quiet, moderately active and active infant have been described in full by the author . . . . Infants of these three types approach similar situations in a different manner . . . a restriction of movement that is frustrating to the active child may not be so to the quiet child, whereas situations which are real obstacles to the quiet type are easily overcome by the active type" (49, p. 86).

<sup>39</sup> " . . . the cradle permits babies who could not otherwise sit up unaided to assume for long periods a position other than that of lying down, out of touch with what is going on around them" (85, p. 25). It might be added that the notion of an Indian cradle as a comfortless prison deserves to be corrected. Nearly all cradles used by North American Indians contained dangling toys and objects to interest the child, foot rests suitable for exercising the legs, etc. (103, p. 162).

even those in which unlimited breast feeding and the most lenient sphincter training are customary, which does not regularly inflict some pain and frustration upon its young. Even childhood experiences which the psychologist and anthropologist view as ideally "satisfying" and "unfrustrating" actually contain frustrating elements.<sup>40</sup> As Goldfrank has pointed out, "Pressures of some sort are exerted during infancy even in those societies which exhibit the greatest leniency in nursing or toilet training, and some degree of permissiveness is present even when infant disciplines are manifestly severe" (56, p. 517).

We can, then, safely assume that infant experience is never uniformly "frustrating" or "satisfying" but always contains a greater or lesser admixture of both elements. The big scientific problem is how to estimate by objective means the extent to which any single situation, as well as the whole of childhood experience, may be considered weighted in the direction of frustration or in the direction of satisfaction. Until this problem has been solved, the efforts of psychoanalysts and anthropologists to explain adult personality in terms of infantile frustrations and gratifications can not be regarded as scientific, since subjective judgment rather than objective procedure determines the weighting of infantile experience.<sup>41</sup>

Dollard and his associates attempt to define frustration objectively as "that condition which exists when a goal-response suffers interference" and, again, "an interference with the occurrence of an instigated goal-response at its proper time in the behavior sequence is called a

<sup>40</sup> E.g., the Hopi, Navaho and Okinawans have been cited as illustrative of societies which afford their children ideal, unfrustrating early experiences. The Hopi child, however, is whipped lightly the first time it is placed on its carrier's back, and it is a regular practice for the mother to place bits of food to the child's lips and then to withdraw this food (56, pp. 521-522); the child is also often kept in semi-darkness, a cloth being placed over the face-guard of the cradle (23, p. 109). Of the Navaho, Kluckhohn reports that "Teasing of children of nine months or a year and over is not infrequent—even by the mother. Sometimes it takes mildly sadistic forms. . . . There are also delays in the response to crying, even of pre-verbal children. A child of seven or eight months may cry fifteen or twenty minutes during the day before a busy mother picks him up" (82, p. 62). On Okinawa, we are told, "The enuretic child is cauterized by placing burning leaves against the skin. Most Okinawan adults carry scars from these fiery applications" (107, p. 394).

<sup>41</sup> John Whiting and Irvin Child have experimented, at Yale University, with the use of independent ratings by observers of the "custom strength" of various childhood disciplines, and of the "habit strength" to which they give rise. The effort to objectify their procedures is commendable, but it runs the risk of substituting a cultural bias for a personal one—i.e., all American raters or all American psychologists may agree in weighting a particular discipline as "severe" or "lenient" in terms of our values; Chinese or Hopi observers, however, might weight the same disciplines differently.

*frustration*" (25, pp. 11, 7). According to this system, a discipline would not be frustrating if it did not interrupt any prior-established goal-directed activity, and by asserting that this is the case in regard to infant restraint, the evidence presented in the preceding section can be brought into line with the frustration-aggression theory. The theory accounts admirably for many experimental observations of infant behavior—e.g., that strength of crying following feeding frustration correlates with the strength of hunger instigation (p. 28). With "frustration" defined, however, the problem is now pushed back to the determination of the strength of instigation and of what constitutes goal-directed activity. Both "instigation" and "goal-response" are defined abstractly,<sup>42</sup> but in practice their determination remains ambiguous in certain crucial infantile situations with which we are concerned;<sup>43</sup> and circular (if no aggression appears, no goal-directed activity has been interfered with) rather than independent reasoning tends to resolve the difficulty for proponents of the theory (as it tends to resolve difficulties for the proponents of most social science theories).

When agreement can be reached that a given experience *is* frustrating to the child, we are still not justified in assuming that an aggressive response is inevitably called forth by the experience.<sup>44</sup>

In Freud's theory frustration as such is supposed to arouse hostility. Actually, however, healthy persons—children as well as adults—are well able to endure a considerable amount of frustration without any reaction of hostility (69, p. 67).

There are any number of frustrations that do not evoke aggressive response in the sense of discharging hostility against a social object or its surrogates. There are, for example, a number of experiments in which animals are frus-

<sup>42</sup> "An *instigator* is some antecedent condition of which the predicted response is the consequence . . . strength [of instigation] is measured by the degree to which the instigated response competes successfully with simultaneously instigated incompatible responses" (25, pp. 3-5).

"An act which terminates a predicted sequence will be called a *goal-response*. The goal-response may be defined as that reaction which reduces the strength of instigation to a degree at which it no longer has as much of a tendency to produce the predicted behavior sequence" (p. 6).

<sup>43</sup> E.g., we are told that even "birth may be an event interfering with responses previously characteristic of the child in its intra-uterine state" (25, p. 58).

<sup>44</sup> Although Dollard and his colleagues have been associated with the view that frustration inevitably leads to some form of aggression, one passage in their work suggests that this formula need not always apply to infants: ". . . frustration can conceivably occur during the birth process itself and at any time thereafter. This need not be the case, however, with aggressive responses as here defined. The first reactions to frustration may indeed be of a random character and may lack that destructiveness which is here posited for aggression" (25, p. 11).



trated and in which such aggression does not occur. . . . The sucking frustrations in infancy cause finger sucking or sucking of other objects . . . rather than increased aggression. There is no proof that the so-called weaning traumas of infancy cause more aggression or even more phantasies of hostility . . . than in other children. The same may be said of all those frustrations that have to do with bowel and bladder control (93, p. 264).

The procedure of some recent analysts of "national character"<sup>45</sup> in dealing with this dilemma of what is frustrating to the child is particularly objectionable from any scientific point of view. It consists of making judgments as to the nature of adult personality and then looking for infant disciplines which can be regarded as having the requisite frustrating or gratifying effect for the production of this personality; or, vice versa, of making judgments about the nature of childhood disciplines and then looking for evidence of personality traits which might be fitted to them. A causal connection is then posited, but not proved. At no point is any search made for evidence which might disprove the thesis, nor are any objective rules laid down which will allow a second party to follow the same procedure independently and achieve the same results. (The analyst is thus safe in his lair, but the progress of science is scarcely advanced thereby.)

One result is that the same childhood experience is arbitrarily read as having one significance for personality formation in one society and the opposite significance in another<sup>46</sup>—the cradleboard in the Southwest e.g., is said to have induced "passivity," but among the Plains Indians, "aggressiveness." Or the same experience *in the same society* is interpreted as having two different significances for personality formation by different observers. Thus Erikson argues that the cradleboard of the Sioux "because it limited the earliest muscle expression may have left a certain inheritance of unchanneled energy to the teething period," and contributed to the "aggressiveness" and "sadism" of the adult Siouan (30, p. 139); whereas "Belden credits the cradleboard for the early development of stoicism in Siouan children, adding that the habit of leaving them bound in these cradles for hours on end, regardless of their crying, teaches them to bear life calmly" (119, p. 12).

#### HOW IMPORTANT IS INFANCY FOR THE STRUCTURING OF CHARACTER?

In previous sections, we have discussed some of the known facts about the relation between specific disciplines of child rearing and the

<sup>45</sup> Particularly Gorer (58, 59), Erikson (30, 31, 32), and La Barre (84).

<sup>46</sup> On theoretical grounds, to be sure, this is perfectly possible; our criticism is directed against the failure to enunciate any rules which guide interpretation.



development of personality. The present section surveys the over-all problem of the importance of the period of infancy in comparison with childhood for the shaping of personality. If it can be shown that major structuring of personality takes place after the first year or year and a half, clearly the contribution of infant experience in general and of these disciplines in particular to subsequent personality formation has been exaggerated by some theorists. We believe that this is the case, and will call attention to some of the evidence that had led us to this conclusion. The picture, however, is not entirely clear, and the honest investigator must confess that apparently discordant facts are faced at every stage of the inquiry. These facts must be reconciled, and not ignored, before any solution of the problem can receive general acceptance. In this predicament, we may draw consolation from the remark of one student in this field that "If a psychologist of personality had to limit his discourse to theories that were securely proved he would have nothing to recount. In his realm there are no certainties" (116, p. xii).

The biologic orientation of Freud's psychology and his concept of unconscious mental processes naturally led psychoanalysts back to the earliest period of infancy in their search for factors influencing personality development.<sup>47</sup> This movement was carried to its logical extension by Rank (124) who viewed the act of birth itself as the basic cause of future anxiety; since all infants experienced the "trauma" of passage through the vaginal canal, every child had some anxiety; especially prolonged or difficult births might produce more severe anxiety states. Greenacre (60, 62) has recently revived this thesis, with some modifications.

It is known that the unborn foetus responds physically to certain stimuli from outside the womb and to some behavior of the mother,<sup>48</sup> but, of course, this furnishes no evidence as to the existence of any correlative emotional or psychological responses in the intrauterine

<sup>47</sup> The approach of biologists to the study of personality may be illustrated by Stockard: "The personality of an animal . . . depends primarily upon its nature or constitution. . . . The biologist very well knows that the most important part of individual existence for man is passed and over before birth" (138a, pp. 21, 26). The concept of the unconscious leads in the same direction, as Bernfeld has observed: ". . . if one completely abandons . . . the equating of conscious and mental, then there is no other way than to trace the mental phenomena back to birth. Perhaps sometime later we may . . . penetrate beyond birth into the foetal being" (11, p. 212).

<sup>48</sup> ". . . following a hard day's work by the mother frequently the child in utero becomes so active, kicking and changing its position, that it is impossible for her to sleep. . . . The presence of fatigue toxin would seem, then, to produce a response in the baby through the mother's blood supply. . . . The response of irritation and restlessness suggests the experience of discomfort for the foetus" (81, p. 185).

state. (But see the apparently contrary view expressed by Dunbar, 28, p. 156.) Freud himself was critical of Rank's thesis, noting that "no trustworthy investigation has ever been carried out to determine whether difficult and protracted birth is correlated in indisputable fashion with the development of neurosis" (47, p. 96); he added, however, that the question was still open. It has been argued against Rank that "individuals who have been delivered by Caesarian section do not, as adults, differ temperamentally in any easily discernible way, from persons who have been normally born" (110, p. 87). However, this statement by Mowrer and Kluckhohn is, apparently, not based upon any empirical study.<sup>49</sup> The only such study of which we know was made by Pearson who compared the behavior of children whose birth took less than six hours with children whose birth took more than fourteen hours. His conclusion was:

There seems to be a difference between the behavior of children whose birth-time was brief and those whose birth-time was long. The former presents somewhat more the picture of the "neurotic" child than the latter, which might indicate that a short birth was a more traumatic experience than a long one. But the duration of birth itself cannot be held responsible solely . . . . The child whose birth is short more frequently is the youngest and had very indulgent parents, while the child whose birth is long is the eldest, the oldest child of the sex or the only child (117, p. 288).

It can be seen that in this case, the relation between duration of birth and anxiety or neurosis was the reverse of that postulated by Rank. But the data available at present are clearly too meagre to allow an empirical resolution of the question, and opinions must be formed instead on the basis of one's general theoretical orientation and on inferences drawn from data that do not bear directly on the subject.

The same can be said about the view of recent proponents of a rooming-in plan for the hospital care of newborn babies that the common hospital practice of separating infant from mother during the two weeks of the lying-in period "definitely interrupts . . . the initial sense of security . . . [and] predisposes these sensitive infants to anxiety" (129, p. 631). At a recent scientific meeting, two advocates of the new plan spoke of the "tremendous" and "enormous" adverse effect which separation from the mother during this period would have on the emo-

<sup>49</sup> Comparative studies of the emotional and nervous reactions of infants delivered by Caesarian and normal birth are referred to by Kenworthy (81, p. 181) who does not, however, cite any supporting data. He observes that "the Caesarian sectioned child is prone to be less sensitized—he cries less, is markedly less irritated by the contacts of handling, etc.—than the first-born child delivered through the birth canal." May one then suppose that the Caesarian child is *not* less sensitized than the *second-born* child?

tional development of the child.<sup>50</sup> Such statements reflect only the subjective judgment of their authors, as is evident from the very form in which they are cast; there is as yet no evidence to support or disprove these assertions (and perhaps that is why the emotions are so frequently resorted to in propounding them).

Analysis of the neonate state has been too much colored by a naive reading-in of adult emotions into the infant which is comparable to the anthropomorphizing views of insect behavior once current among an earlier generation of biologists. "When a child comes into this world, he comes in lonely and he is afraid," one psychiatrist declares (9, p. 582); another physician writes that the newborn is "a frightened, primitive little creature" (2, p. 593). Especially in psychoanalytic literature, it is often "stated quite confidently that the child at birth is capable of profound and intense feeling" (75, p. 752). We agree wholeheartedly with Hendrick's comment that these descriptions of the neonate represent "far more projection of analytic theory and adult passions than scientific observation" (66, p. 33).

To some extent, of course, inference is the only possible means of approximating to an understanding of the psychological condition of the newborn infant. But this inference should rest upon concrete observation of neonate behavior rather than upon preconceptions as to the nature of the neonate state or extrapolations from a later period. The best observations we have to date indicate that the newborn infant knows neither anxiety nor confidence, fear nor happiness, but exists in an affectless and presumably consciousless state. Its behavior is characterized principally by a great amount of undifferentiated mass activity (74, p. 39). Its "resistance to many types of stimuli is very high; even during handling the infant will often fall asleep" (137, pp. 74-75); most of its day is spent sleeping. "When an infant below four or five days of age is dropped one or two feet it frequently shows no perceptible response. . . . This is also true for so-called "pleasurable" stimuli, such as stroking or petting, to which many newborn infants show no overt reaction" (137, p. 145). Efforts to establish a conditioned response in the neonate have met with failure or the most equivocal results.<sup>51</sup>

<sup>50</sup> We refer to addresses given by Margaret Mead and Edith Jackson at the Spring Meeting of the Society for Applied Anthropology in New Haven, May 30, 1947. Cf. Jackson *et al.* (74a) where more temperate claims for the virtues of the rooming-in plan are made.

<sup>51</sup> Marquis (99) contends that she set up a conditioned response of foodtaking reactions to the sound of a buzzer in 7 out of 8 newborn infants during the first ten days of life. Pratt, however, has rejected her claim as invalid on technical grounds (122, p. 239) and, after a thorough review of the literature, concludes that "There is some evidence

Nevertheless, it has been observed that "research workers, as well as nurses and parents, are inclined to recognize personality, in terms of individual characteristics, as early as the first day of life" (78, p. 4). Jones and Burks are probably correct in attributing this early recognition of individuality to "differences in muscle tone, motor activity, sensory thresholds, [and what is interpreted as] emotional expressiveness . . ." (78, p. 5). Certainly there are important physical differences between infants at birth "in the maturity of the nervous system, in the way frustrations are overcome, in the response to the Moro test, and in the amount of activity" (48, p. 226). Fries divides newborn infants into three types, in accordance with the amount of activity displayed: "the quiet, the moderately active, and the very active" (48). Just how such physical differences correlate with later personality remains uncertain,<sup>82</sup> although offering a promising field for investigation. Whether one designates the physical characteristics of the neonate as its "personality" or whether one chooses to restrict the term to emotional and psychological phenomena which arise only in later months is purely a matter of definition.<sup>83</sup>

In the same ill-founded but influential article referred to previously, Watson and Morgan declared that the newborn infant evidenced three basic emotional reactions belonging to "the original and fundamental nature of man: fear, rage, and love" (143, p. 165). The "fear" response consisted of "a sudden catching of the breath, clutching randomly with the hands . . . blinking of the eyelids, puckering of the lips, then crying"; it was called forth by falling, loud sounds, or a sudden push (p. 166). The "rage" reaction, produced by restraint, has been discussed in a previous section of this paper (*supra*, pp. 21-24). The "love" response was evoked by "stroking, or manipulation of some erogenous

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that certain responses may be conditioned to experimental stimuli during the neonatal period. The responses are difficult to establish, highly unstable, and cannot be set up in all infants" (p. 240). His view is echoed by Wenger. "Conditioning in the neonate is unstable and not easily obtained . . . anticipatory mouth opening occurs at least by the second month, but evidence of conditionability before that time is equivocal" (145, p. 85; cf. 146 and Munn, 112, p. 376).

<sup>82</sup> Fries declares: "There seems to be a high correlation between amount of activity [in infancy] and ability to meet a new situation or to overcome an obstacle, deprivation or frustration" (48, p. 228). Support for her position comes from Davis and Havighurst's study of 200 Chicago children. They found that "The children who were most active when young, compared with those who were most quiet when young, tend to be: most active now, most punished, more aggressive, and less neat" (18, p. 706).

<sup>83</sup> Allport, e.g., holds that "the newborn infant *lacks* personality. Although many of its determinants are congenital, personality as such is not inherited. Only when the original stream of activity meets the environment, acting upon it and being acted upon by it, do the first . . . incipient traits emerge" (5, p. 122).



zone, tickling, shaking, gentle rocking, patting" (143, p. 167). From these three initial emotions, the entire gamut of adult emotional expression was presumed to develop by conditioning and extension. It can be seen how Watson's theory complements the extreme psychoanalytic view of the neonate as an organism whose personality can be permanently structured by very early experience and conditioning, and how closely the writings of Ribble parallel this early statement.

Just as his view of the infant's rage reaction to restraint has been rejected, however, so must the remainder of Watson's theory be dismissed as unsatisfactory. It overlooked "the possibility that some emotional reactions might not be elicitable at birth and yet appear later on as a result of maturation rather than as a product of conditioning" (122, p. 235). Behavior which Watson claimed to have observed in response to specific stimuli was not confirmed by subsequent tests.<sup>54</sup>

The most convincing attack upon Watson's theory was made by Sherman (137, pp. 114-143) whose observations also illuminate the subject of adult reading-in of emotion into the child. He showed motion pictures of infant behavior to a group of graduate psychology students and asked them to name the emotion represented, the stimuli which produced the behavior having been deleted. No agreement was exhibited in naming the emotions. Similar conclusions were reached when judges were brought into direct contact with the infants (the stimuli which Watson used to evoke "fear" and "anger" reactions were administered to the infant behind a screen, and the screen was then removed so that the students could judge the reaction). Sherman's conclusions deserve to be underscored: *"In estimating the emotional behavior of infants, people tend to read into it their own attitudes toward the expected reaction. . . . Frequently we ascribe to the child reactions of which he is not at all capable. . . . Most persons judge the emotional behavior of an individual in terms of the stimuli which have produced the reactions"* (pp. 122-123, 142).<sup>55</sup>

<sup>54</sup> See, e.g., Sherman's observation quoted above on p. 30; and the work of Dennis cited in the section on restraint. Repeating Watson's test for "fear," Irwin dropped infants two feet; only two of 82 trials results in crying, no response was noted in 12 per cent and various limb movements were observed in 88 per cent of the cases (73, p. 168).

<sup>55</sup> One limitation should be noted in regard to Sherman's study. The behavior ("emotions") which he asked judges to identify—"hunger," "anger," "pain," and "fear"—was all produced by disturbing stimuli. Had he attempted merely to distinguish general states such as "excited" and "quiescent," the judges' agreement would doubtless have been near-unanimous.

A study by Goodenough (57) found that university students rated almost six times better than chance in judging emotions represented by photographs of a ten month old child, but at this age a greater ability to communicate (and detect) emotion from facial expressions would be expected.



As a result of Sherman's work, many child psychologists have gone so far as to abandon the word "emotion" in referring to behavior of the young infant. "In its place they advocate a purely descriptive account of the extent of organismic involvement in a response under definite conditions of stimulation, age of organism, and so on" (122, p. 236). The consensus of opinion of competent students, summarized by Pratt after a comprehensive review of the literature in this field, is that "It seems certain that *such well coordinated reactions as those implied by the terms love, rage, and fear, do not exist at this early age, [i.e. in the neonate]*" (122, p. 211, our italics). The present view regards emotions as developing gradually in the infant by a process of differentiation from generalized to more specific reactions (p. 208). A study by Bridges (12) of the emotional behavior of 62 infants in a Montreal hospital, for example, distinguishes only two amorphous states at one month—"excited" and "quiescent." At three months "distress" and "delight" are differentiated, and it is only subsequently that "anger," "fear," and "affection" are manifested. One may reject this particular developmental sequence or terminology, but the fact of differentiation from initially generalized responses and the importance of maturation<sup>55</sup> in the development of emotions seems to be generally conceded.

The point we would emphasize is that, in the light of the findings of child psychologists, there does not appear to be any justification for speaking of emotions such as "fear," "anxiety," or "love" in the infant during the first months of life, as is so frequently done. Only confusion can attach to the designation of the physical states of early infancy by emotional terms which derive their meaning from a later period of life.

Although, as has been stated, conditioning is difficult to effect in the first weeks of life, some learning—especially in relation to the feeding situation—is definitely established for the first month of life. Thus a study of the feeding response of infants at a New Haven hospital showed that

... the human infant does, within the first weeks of life, modify his behavior in accordance with the external demands imposed upon him by the culture into which he is born. Infants on a three-hour schedule "learned" to be "hungry" at the end of three hours, those on a four-hour schedule showed less definite, but

<sup>55</sup> "The varying effects of a given external event as children grow older can ... be noted in occasional shifts in emotional response. At about 5 to 8 months, for example, a hungry but hitherto relatively calm infant may show signs of anger if there is a momentary delay on his feeding after he has caught sight of the bottle. ... Similarly, as a child's abilities increase with age, an event that previously frightened him may provoke anger, and at a still later time the event may provoke amusement" (75, p. 758).

certainly suggestive, evidence of having learned to wait four hours for feeding (100, p. 277).

During subsequent months, a whole range of conditioned responses can be developed, which evidence greater stability as the infant ages; "retention of conditioned responses developed in infancy in some instances have been observed over periods ranging up to 7 months" (112, p. 376).

Two interesting experiments performed upon rats by Hunt (70) and Wolf (148) lend considerable support to the view that infant experience is an important determinant of adult behavior. Hunt subjected one group of rats to food deprivation for a period of 15 days starting at the age of 24 days, and a second group to a similar period of deprivation starting at the age of 32 days. Both groups were then given unlimited feeding for five months, after which they were again subjected to a five day period of starvation, together with control animals who had not been deprived during infancy. The experimental rats in the 24-day group responded to the second period of deprivation by hoarding more than two and a half times as many pellets of food as did the control rats who had not been starved in infancy; but the 32-day group did not show any excess hoarding. Hunt interprets these results as showing that early infantile frustration has a greater effect upon adult behavior than does later infantile frustration (70, p. 359), a conclusion which would extend to the psychological sphere an observation that has been established in the biological (especially the embryological) sphere (see Stockard, 138a, pp. 45, 134 ff.). This, of course, is the firm logic behind the psychoanalytic position.

Wolf's excellent experiment (148) is of equal interest. He impaired the hearing of one group of rats by taping their ears and the sight of a second group by taping their eyes for several weeks during infancy. When the restraints were removed, the animals learned to see and hear normally as adults. But placed in a situation where they had to compete with control animals for food in response to a light stimulus, the animals blinded in infancy experienced difficulty in seeing and failed to get the food; when a sound stimulus was substituted, the animals who had been deaf during infancy similarly failed. Animals whose functions were impaired for a period *after* infancy (and then restored), were not handicapped in the exercise of these functions during the competitive situation (pp. 33-34).

Apart from the consideration of the applicability to a larger animal of findings made upon a smaller one, a question raised by these experiments, as Murphy has observed (114, pp. 308-309), is the extent to

which reactions to a specific infantile frustration can be generalized and transferred to other situations. Murphy expresses the belief that

Studies on the influence of infantile frustrations upon subsequent drive and behavior structure . . . suggest a certain transfer and generalization to a genuine "personality configuration" in the adult, whether animal or man . . . we may say that continuity is no longer merely the continuity of an item; it soon becomes the continuity of a system (114, pp. 723-724).

Linton has suggested that certain generalized responses are easy to establish in the early years of life and hard to establish later, in contrast to specific responses which may be acquired at any time during the life cycle (95a, p. 116).

If correct, the facts of (1) infantile conditioning showing up in adult behavior and (2) being generalized to a "personality configuration," are not necessarily contradictory to the position we have taken previously. The main question remains: does experience during the first year structure personality so firmly that it can not be restructured by later experience, or is the continuity of personality to be explained rather by the continuity of later experience, and of the constitution, with that of the first year?<sup>17</sup>

Probably no one answer to this question can be given for the entire range of possible infantile experience. If one burns a die upon the infant's body, its stamp will endure throughout life. Perhaps there are intense and prolonged forms of experience during the first year which can similarly cast the infant's personality into a mold which cannot be reshaped by any subsequent events.

In the normal range of infant experience, however, we believe that events subsequent to the first year or two of life have the power to "confirm or deny" the personality of the growing infant, to perpetuate or remake it, depending upon whether the situation of later childhood perpetuates or alters the situation in which the infant was reared. Since this is one of our most crucial theses, and bears heavily on the problem of the relative importance of cultural as against instinctual factors in

<sup>17</sup> " . . . several studies . . . demonstrate *persistence* of personality patterns (occasionally even in the face of active efforts on the part of parents or others to break up the patterns). . . . For illustration we may mention Washburn's study showing constancy of smiling and laughing during the first year of life, and a study by Jersild and Marky which reports a correlation as high as .79 to represent the constancy of 'fights and quarrels' of pre-school children from one year to the next. . . . Persistence of the trends may be due merely to persistent factors in the environment . . . [or] early influence or experiences which set the whole future course of development" (78, pp. 93-94).

Cf. Benedict (10), Goldfrank (56), Beaglehole (8), and Linton (95a, pp. 141-145) for theoretical formulations regarding the effect on personality of cultural continuity or discontinuity in childhood training.

— personality formation, we will devote some space to its documentation.

The picture of the infant obtained from many psychoanalytic accounts has much in common with the medieval notion of the infant as a homunculus or miniature adult. A scientific picture of the infant during the first year of life, however, does not support this notion, as Allport makes clear:

[During the first year] . . . there are certain conditions that prevent the formation of dispositions as stable as those to be formed later. There is, for example, a low degree of retentivity for conscious experiences. . . . Then too, though the infant learns rapidly, he also forgets rapidly. Habits . . . are readily lost. . . . The propensity for an all-or-none type of emotional activity prevents his learning discriminative affective responses or the development of a hierarchy of likes and dislikes. His capacity for conceptualization is slight. There are, then, plenty of reasons why personality should be less stable, less predictable, and less consistent in the early months of life than at any later time. It is indeed never again as unorganized and unstructured as in infancy (5, p. 130).

The following evidence suggests the vital importance of post-infant experience to the formation of personality:

1. The personality of adult Pueblo Indians is marked by "maladjustment" and "apprehensiveness," despite an infancy characterized by unlimited indulgence (56).

2. "Navaho Indian infants receive a maximum of protection and gratification, but the anxiety level among adults is very high—presumably because of post-infancy traumas" (110, p. 96). "Navaho children who leave the hogans calm and well-poised return at the end of the first school year nervous and tense . . . the theorists . . . claim too much for the earliest years and do not pay enough attention to later events and to the total situation in which the mature person finds himself" (85, pp. 68, 111).

3. ". . . the Kaska personality norm is introvert and the Haitian extrovert, although in both societies babies are fed when they so desire, receive no pressure to control elimination, are comforted and handled when irritable or playful, and suffer no discipline for emotional willfulness. The indication is that other criteria than those associated with physiological urges are important in personality structuring, and moreover, that these may lie in the childhood rather than infant experiences" (142, p. 575).

4. St. Thomas children of the lower economic class are reared with great indulgence: "Breast feeding is ordinarily continued for ten or twelve months and not uncommonly for as long as two years. No regular feeding schedule is held to; the child is fed when it cries. . . . The training of sphincter control is not begun until relatively late, often not until the age of three or four years and very unsystematically then." Nevertheless, "there typically emerges an insecure, suspicious, egocentric adult" (14, pp. 43, 89).

5. "The pathogenic effect of later impressions is firmly established, and it can be assumed without danger that a severe traumatic influence during the war, for example, might set up a traumatic neurosis in a person whose birth had been quite normal. Indeed, under a sufficiently strong traumatic influence, the healthiest of individuals can become neurotic" (4, p. 114).



6. Some American ethnic groups "were giving children more of the kind of warmth and consistent mothering which psychiatry and pediatrics have recently come to consider desirable than were the completely American groups. . . . The unspoiled maternal attitude has much to counteract, however, when it confronts the instability of depression years . . . a satisfying infancy does not necessarily compensate for economic deprivation in the next ten years, and there is increasing evidence that later gratification may go far toward offsetting the effects of early frustration" (115, pp. 656-657).

7. In most societies, male and female infants are treated substantially alike for the first year or two of life, but differences in behavior are increasingly manifested in childhood and later years. Whatever behavioral and psychological differences exist between adults of different sex, then, must be attributed to biological factors and the differential cultural conditioning of post-infant years.<sup>88</sup>

Faced with evidence such as that cited above, some of which they have contributed themselves, Mowrer, Leighton, and Kluckhohn attempt to salvage a part of Freudian theory by reasoning that

Infantile indulgence probably does constitute the firmest foundation upon which, if later circumstances are reasonably favorable, a secure and confident adult personality can be developed. But it affords only a possible basis; it does not, in and of itself, promise fulfillment . . . infant experiences, while placing certain constraints upon personality, give mainly potentialities. . . . Whether these potentialities become actualized or not . . . depends upon later social conditions . . . only an external condition of adult life brings out fully a predisposition, the basis for which has been laid in the experiences of infancy and early childhood (85, p. 11; 110, pp. 95-96).

Assuredly, this formulation is a vast improvement over the simplistic theories of the relation between infant discipline and adult personality which are still in vogue. It is reminiscent of the compromise between the instinctual Freudian and the social-situational views of anal personality genesis which Fenichel advanced (35). As Fenichel spoke of the anal "desire to collect something," so these authors speak of adult "predispositions" and "potentialities" as "determined" by infant experience. Perhaps they are correct, but as there is still no way to measure a "predisposition" before the eventual empirical psychological disposition has been made, we wonder if there is not more mystification than meaning in the use of such terms. If infantile indulgence does not necessarily produce a confident personality, and if later gratification may offset earlier frustration, the range of "potential" personality is,

<sup>88</sup> "Emotional differences between the sexes have not been observed during early life although boy infants are believed by many to be more difficult to manage than girls. Girls talk somewhat earlier than boys but in other respects mental development proceeds at the same rate in both sexes. The greater aggressiveness of boys . . . becomes evident during the second or third year of life" (Bakwin, 7, p. 46). Cf. Marshall (101, p. 12).



in any case, too large to be fruitfully delimited on the basis of present knowledge. Instead of introducing teleological terms, it would seem preferable merely to describe the nature of organism and environment as precisely as possible at all stages in their history of interaction—infancy, childhood, adolescence, and maturity—allowing for the likelihood that (a) different infantile experiences acting upon different constitutions may produce the same personality type, (b) the same infantile experience acting upon different constitutions may produce different personality types, (c) the same childhood experience following different infantile experiences may produce similar personality types, (d) different childhood experiences following the same infantile experience may produce different personality types.

We conclude that the rigidity of character structuring during the first year or two of life has been exaggerated by many authorities, and that the events of childhood and later years are of great importance in reinforcing or changing the character structure tentatively formed during infancy.<sup>59</sup> Or one may substitute Horney's formulation: "... the sum total of childhood experiences brings about a certain character structure, or rather starts its development. With some persons this development essentially stops at the age of five. With some it stops in adolescence, with others around thirty, with a few it goes on until old age" (69, p. 152).

#### DISCUSSION

It can be conceded that social scientists have failed to produce a definitive answer to the question of the relation between infant disciplines and character development, because of a general lack of historical and cultural sophistication, the difficulty of establishing the validity of the personality measurements employed, and the difficulty of isolating single factors for study. It is hard to see how the last obstacle, in particular, can be overcome. Social phenomena can not readily be subjected to the type of crucial experiment which enables

<sup>59</sup> Beaglehole (8) has proposed a useful distinction between the "primary" character structure formed in infancy, and the "secondary" character structure which may be formed in later years: "The integration-determining structure of needs and feelings constructed during infancy is generally reenforced and supported by the continued conditioning of the middle years of childhood and adolescence. . . . Hence this original character structure may very well be called the primary character structure of the person. Under normal circumstances and in relatively static societies, it will be the only character structure that the person possesses. In certain societies, however . . . the person may very well build or be forced to develop, either for permanent or temporary change, a secondary character structure which will enable him as a person to react dynamically and adaptively to a new set of major directives" (p. 149).

the scientist to support or discredit an hypothesis. One has the feeling that social science theory, therefore, often moves in cycles like fads or persists like customs, for historical reasons, rather than progressing firmly in one direction because certain truths have been established by objective tests and can be built upon by the exercise of reason.

If the personality theory before us be of this nature, then the task we have undertaken in this paper of subjecting it to empirical test is indeed hopeless. Out of an obstinate spirit, however, let us proceed *as if* this were not the case.

Accepting, then, the data which have been reviewed as tentative markers on the psychological terrain, we are led to reject the thesis that specific nursing disciplines have a specific, invariant psychological impact upon the child. Instead, it appears that the effect of a particular discipline can be determined only from knowledge of the parental attitudes associated with it, the value which the culture places upon that discipline, the organic constitution of the infant, and the entire socio-cultural situation in which the individual is located. In short, it is contended that personality is not the resultant of instinctual infantile libidinal drives mechanically channelled by parental disciplines, but rather that it is a dynamic product of the interaction of a unique organism undergoing maturation and a unique physical and social environment. This view is in substantial agreement with the position which Horney (69, p. 70) and Fromm (51, p. 286), among others, have advocated. Aside from meeting the facts of personality formation better than does orthodox Freudian theory, it has the advantage of being able to relate character structure to a concrete historical situation, which Freudian theory has been unable to do:

As long as we assume, for instance, that the anal character, as it is typical of the European lower middle class, is caused by certain early experiences in connection with defecation, we have hardly any data that lead us to understand why a specific class should have an anal social character. However, if we understand it as one form of relatedness to others, rooted in the character structure and resulting from the experiences with the outside world, we have a key for understanding why the whole mode of life of the lower middle class . . . made for the development of this kind of character structure (51, p. 293).

In an illuminating passage, Murphy has pointed out that "there is no such thing as the 'true' or 'real' attitude, independent of the human situation. There are indeed many *potential* attitudes which are real, normal, human, and important, but none can appear and function except in a certain situation. Personality can be defined in terms of situations. . . . Personality could almost be defined as the integration of all the roles that a particular person has to enact" (114, pp. 876-877). If

this is true of attitudes, must it not be equally true of infant experience, that no experience can be meaningfully isolated from the biological-psycho'logical-cultural context in which it is embedded; that no discipline has an automatic, invariant emotional effect upon the infant? There is nothing new in this idea. Fromm (51, p. 293), Horney (68, p. 80), Kardiner (80, p. 224), Fries (50, p. 14), Lois B. Murphy (115, p. 654), Mowrer and Kluckhohn (110, p. 96) have all said the same thing at various times.

A particularly clear statement of this view was made by Pearson in 1931, after an intensive study of 72 problem children at the Philadelphia Child Guidance Clinic. The question which Pearson posed was whether the child's relationship to the parents, or the physiological events of its life were more important in shaping its personality. Under "relationships to parents," the attitudes of each parent toward the pregnancy, toward the child, and the position of the child in the family were determined; under "events," the health of the mother during pregnancy, the duration of pregnancy, length of birth, duration of nursing, duration of weaning, method of weaning, and method of bladder and bowel training were recorded.<sup>60</sup> His conclusion was that:

... the parental attitudes exert a more important influence on the formation of the child's personality than the actual events. . . .

There is no question that the physical situations of a child's life bear a definite relation to the formation of his personality, but the number of associated etiological factors renders it impossible to consider any single situation as causative. This study does indicate, however, the marked importance of parental attitudes. . . . The child forms most of his ideas about the world of humanity from his impressions of his parents, and their attitude to him . . . must affect the formation of his personality to a far greater degree than the length of his birth or the duration of his breast feeding. The events of a child's life enhance and crystallize the parental attitude toward him . . . and the child reacts to the parental attitudes through its association with the events of his life rather than to the events themselves (117, pp. 287, 290-291).

If this hypothesis is correct, the importance which Freud ascribed to traumatic events of childhood in structuring personality was greatly exaggerated. Several investigators have reached the same conclusion.<sup>61</sup>

<sup>60</sup> Cf. Davis and Havighurst (18) who, interviewing 200 well-sampled Chicago families, recorded similar data on child training and personality with the object of testing for any correlation between the two. Their conclusion was negative: "Very few pronounced relationships appeared" (p. 709).

<sup>61</sup> E.g., Sears: "Several sources of evidence indicate . . . that Freud seriously overestimated . . . the importance of childhood sex aggressions . . . childhood [sexual] experiences are outlawed in our own culture and the child who has them, either willingly or unwillingly . . . is made to feel guilty or ashamed . . . the guilt and anxiety may in some instance lead to actual neurotic symptom formation. This would naturally be attributed to the experienced *event* rather than to the *emotions* involved" (136, pp. 36-37).

Assuming that the personality of the mother and her attitude toward the child are major factors in shaping the child's personality, the problem arises of when and how their influence is first manifested. Once the infant recognizes the mother as a distinct individual, let us say at the sixth month, significant interpersonal relations can certainly be said to have started. These earliest relations, however, should not simply be considered to affect the infant in the precise manner that an older child might be affected by the same relations. What may be called an "aberration affect" probably colors the experience for the infant until it is brought into sharper focus by maturation and learning.<sup>63</sup> Clearly, moreover, no fixed time can be set for the child's first conscious perception of its environment; and the annoying problem of unconscious perception remains. But to overemphasize the role of the unconscious in the first few months of life leads to the error which, we believe, Rank committed, and flies in the face of the descriptive data of infant psychology which have been summarized above. It is important to stress that the neonate and young infant is an immature animal organism, culturally "neuter" and psychologically uncommitted (to the extent that its constitution has not committed it), which can only slowly and with much parental effort and the gradual maturation of its faculties, be socialized. When Freud writes that little children "cannot help conceiving the sexual act as a kind of maltreating or overpowering, that is, it impresses them in a sadistic sense" (45, p. 55), he gives us insight into the Viennese but not into the infantile mind. Some anthropologists who regard the infant as a cultural being from the moment of birth—even of conception<sup>64</sup>—commit a similar error in failing to recognize that, although the neonate is instantly enveloped in a world of culture, it takes a considerable period of time until important elements of this culture are introjected, and the maturing infant remains, therefore, primarily a culture-less animal.<sup>64</sup> The ethnocentric, anthropo-

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Cf. Linton: "... the potentialities of . . . [traumatic] experiences for affecting the personality are probably determined quite as much by the attitudes of other individuals toward the incident as by any intrinsic qualities of the incident itself. Since these attitudes are primarily determined by culture, even this type of experience is culturally influenced" (94, p. 469).

<sup>63</sup> E.g. "... the very young child seems unable to recognize difference between facial expressions . . . babies three months old may respond to scolding with definite signs of amusement" (16, p. 108).

<sup>64</sup> Statement of Margaret Mead at the May 29, 1948 session of the Society for Applied Anthropology in Philadelphia.

<sup>64</sup> How watery is the "culture of infants," can be judged from an interesting article which attempts to itemize the traits that infants of various ages are expected to acquire in the United States (Harlan, 64).



morphic view is the most difficult error to guard against in studies of infant psychology.

Many writers speculate about a process of "empathy" or "emotional contagion" whereby the young infant senses the mother's attitude to it by means of subtle physical expressions, body tensions, or some less material signs.<sup>65</sup> At times the speculation hovers on the borders of mysticism; we have seen little evidence that such empathetic transfer takes place in the very first months of life.<sup>66</sup> The supposition is intriguing, however, and if it can be reduced to behavioral terms and confirmed, it would provide a vital missing link in the mother-child chain of personality communication. There is a good deal of evidence that subtle behavioral cues to maternal emotion are detected by the child in later months of life,<sup>67</sup> and that these cues may be more important in governing its character development than are the gross patterns of discipline which an observer may quickly note.

#### SUMMARY

This paper reviews some of the empirical data bearing on the theory that various features of infant care determine adult personality. Our conclusion has been largely negative, and we have been led to substitute a theory which emphasizes, instead, the importance of constitutional factors and of the total cultural situation in personality formation; the importance of post-infantile experience is also indicated. To the criticism that this paper stops where it should start, with the definition of specific situations which can be calculated to produce specific psychological effects upon individuals with certain constitutions, we can only agree.

<sup>65</sup> See Dunbar (28, p. 157), Escalona (33, p. 80), Greenacre (61, p. 209), Lundberg and Farnham (97, p. 305), Maslow and Szilagyi-Kessler (102, p. 84), Mullahy (111, p. 178), Plant (120, p. 96), and Sullivan (139, p. 8).

<sup>66</sup> Some of the most convincing evidence concerns the reported transfer of food aversions from nurse to infant. Escalona (33) notes that infants' acceptance or rejection of orange or tomato juice depended upon which juice the person feeding them liked or disliked, as well as upon the feeder's attitude to the infant. Similarly, Dunbar declares that "there is probably no point in prescribing for the infant any food substance for which the mother, nurse, or whoever is in charge of feeding, has a definite dislike" (28, p. 157).

<sup>67</sup> Dunbar, however, suggests that the psychological state of the mother can even be communicated to the unborn foetus: "The physiology of the mother is changed when she is under emotional strain and the effect of these changes is transmitted to the foetus through the placental circulation and in other ways" (28, p. 156). Cf. Greenacre (60).



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Received August 30, 1948.

## A NOTE ON GRANT'S DISCUSSION OF THE LATIN SQUARE PRINCIPLE IN THE DESIGN OF EXPERIMENTS

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The suggestions made by Grant<sup>1</sup> that researchers might find a variety of uses for the Latin and Greek square arrangements of variables are especially to the point for psychologists concerned with learning problems in which a series of materials is learned in succession by the same subjects. Although Grant points out the similarity of principle between the Latin square and the traditional ABBA arrangements used commonly to control practice, fatigue, and other cumulative variables, there are some features of special interest in rote learning experimentation that were not brought out in Grant's general treatment.

*The learning of a single list of nonsense syllables.* The problem of which items in a single list are learned first, last, etc., is usually attacked by use of a list containing syllables of allegedly equal association value. Although individually the syllables may meet some criterion of low association value it may turn out that a given syllable is more or less easily learned depending on which syllable it follows. The writer has accumulated evidence to show that a given syllable has different learnability depending not only on its position in a list but also on what syllable it follows. Any claim made about positional effects based on the use of one set of syllables is open to criticism. The obvious corrective is either to use many different lists or to use the same syllables in different orders and arrangements so that each syllable follows every other syllable equally often, and in each and every ordinal position. Thus a six-syllable list could be expanded into a latin square consisting of six lists in which each column represents one arrangement showing the order in which the separate syllables appear, as follows:

1	2	3	4	5	6
2	4	6	1	3	5
3	6	2	5	1	4
4	1	5	2	6	3
5	3	1	6	4	2
6	5	4	3	2	1

In this square it is apparent that each syllable appears in each position only once and that it always precedes and follows a different syllable in each list (columns). In this way any combination of syllables that would tend to facilitate the learning of either or both is uniformly

<sup>1</sup> GRANT, D. A., The latin square principle in the design and analysis of psychological experiments. *Psychol. Bull.*, 45, 427-442.

distributed among the subjects. Each syllable also appears in each position thus controlling any advantage that might accrue to a syllable from position alone.

*The learning of successive lists of syllables.* In this type of problem a subject may be asked to learn one list of syllables or problems after another. The several lists should be thought of as comparable to individual nonsense syllables in a single list and the lists should then be arranged as above to avoid the possible cumulative effects of some one problem or list acting more positively in some other problem or list, thereby giving a spurious effect which might be attributed to position or to some other variable. In a proactive inhibition experiment, for example, it might make a difference whether list 4 follows list 5 or 6 or some other list, and if such other list is chosen at random, the results may be open to a cumulative effect error.

*Odd-numbered vs even-numbered squares in experiments with nonsense syllables.* Some of Grant's illustrative examples are taken from the field of agricultural experimentation wherein a number of variables are simultaneously counterbalanced by the Latin square technique. Under such circumstances it does not matter which item follows which in rows or columns since cumulative effects are not being considered. In such a circumstance, the Latin square shown by Grant

A	E	B	D	A
E	A	C	B	D
B	D	E	A	C
A	B	D	C	E
D	C	A	E	B

is apparently appropriate since each item appears in each row and in each column. It should be noted, however, that such an arrangement is not appropriate for nonsense syllables either in one list or in a series of lists because some of the items follow particular items more than once; thus if A has a specific effect on D, such an effect will occur twice because D follows A in column 1 and in column 5; again E follows C twice, B follows E twice, A follows B twice, etc. A little experimentation will demonstrate that it is impossible to arrange any odd-numbered set of items into a square without this difficulty, whereas there is no such problem with even-numbered lists. On page 439 in a foot-note discussing the Greco-Latin square, Grant states that a square of such a nature could not be developed with 6, 10, 14, and 18 unit sets of variables; this might be misconstrued to imply that experimenters should work with sets of lists or lists with odd numbers of items, whereas the contrary is true with the Latin square if cumulative positional effects are to be avoided.

*Received December 8, 1948.*



## NOTE ON THE POSTMAN-STONE CONTROVERSY

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Those readers of the *Psychological Bulletin* who are interested in the present status of Thorndike's law of effect have been provided with entertainment, if not information, by the year-long exchange of "notes" and "replies" between Postman and Stone (4, 5, 6). Since this exchange has reached the point where it consists largely of emotionally-charged words and phrases, it is perhaps time for a hitherto uninvolved party to risk an attempt to reconcile their divergent viewpoints.

Thorndike's law of effect, in its 1932 reformulation, served to split psychological opinion concerning the role of punishment in learning and to spur experimental studies. Postman's bibliography of 332 titles (3) attests to the vigor and enthusiasm with which that problem was attacked. However a half-century of extensive and intensive experimentation has served only to increase the divergence of psychological opinion relative to the effect of reward and punishment on learning. Both proponents and opponents of the law of effect have accumulated stores of research ammunition to justify their positions. Experiments may be cited to back the position that punishment helps, hinders or has no effect on the elimination of errors (e.g., 1), and this is true of reward as well. There seems to be experimental evidence available as "proof" of any pet thesis which any individual may care to state as a "law," including the four logical possibilities suggested by Stone (5, p. 154).

Thorndike's law of effect in particular has not stood the test of time and research in terms of achieving the universality implicit in its formulation. Nor, in the opinion of the writer, can it withstand the test of common sense and everyday experience. If each of the trio involved in the present discussion were to burn his fingers in a new and unfamiliar type of fire, it is to be doubted if their later avoidance of that fire would have the indirectness of the creature who, according to Thorndike's "law," is "led by the annoying after-effect to do something else to the situation which makes him later less likely to follow the original connection" (7, p. 71). Human behavior, even the behavior of psychologists, tends to be somewhat more complex than that of the schematic sow-bug.

Does this mean that any effort to generalize within the important

area of reward and punishment is hopeless? Not necessarily. It depends, to a considerable extent, upon how wide a generalization is attempted. It appears probable that any attempt at a "law" regarding "the effect" of reward and punishment on learning in general is doomed to failure. Available evidence suggests that reward and punishment do not have a single effect upon all types of learning under all conditions, but rather that at least four "effects" may be involved depending upon the specific situation. Tolman, Hall and Bretnall (8) have presented evidence for "laws" of emphasis, motivation and disruption, and evidence has also been produced which points to an informational value of such punishments as are applied in contiguity with errors (e.g., 2).

It we recognize that any of the above effects may take preeminence in a specific learning situation, and that combinations of effects may operate simultaneously, we will have gone a long way toward reconciling divergent viewpoints and toward understanding the differences in results obtained experimentally. We will have recognized that the effect of reward or punishment in a learning situation depends on the specific nature of the reward or punishment, the specific learning situation involved, and, not least of all, the specific individual being rewarded or punished. Adherence to the law of parsimony, seeking for a single "truth" or "law" applicable to all situations, is admirable. But persistent, obstinate adherence to a generalization which has failed of its intended universality is illogical and unscientific.

Thorndike's generalization, popularized as the "law of effect," has a firm foundation in experimental research. However many other researches, just as competently planned, executed, and interpreted, yield results not in accordance with this generalization. To the writer, this suggests that a generalization as wide as the law of effect is not feasible. Perhaps several "laws of effect" are called for, each to describe results obtained within a narrower sphere. In the light of available data, neither battles of quotations nor battles involving the situation of selected confirmatory experiments serve a useful purpose. It may be more profitable to recognize the contradictory nature of the available evidence, recognize the possibility that at least to some extent

"I am right, and you are right,  
And all is right as right can be,"

and seek generalizations which will *fit* all the available data more adequately than does the law of effect.

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## BOOK REVIEWS

BORING, E. G., LANGFELD, H. S., & WELD, H. P. (Eds.). *Foundations of psychology*. New York: Wiley, 1948. Pp. xv+632.

*The Foundations of Psychology* is the successor of the editors' previous elementary texts: *Psychology: a factual textbook*, 1935, and *Introduction to Psychology*, 1939 (both reviewed in *THIS JOURNAL* by the writer: 1935, 32, 595-599; and 1939, 36, 784-786). Although the editorial board remains the same, fifteen of the present eighteen contributors are new. It is no longer true, as it was for the previous editions, that most of the authors are "descendants" in the Titchenerian tradition. The present group of authors shows a great diversity of theoretical and professional background, and their writing well justifies the editors' statement that "In 1948 the important thing about the organism is not that it is conscious, but that it reacts to stimulation."

The sequence of chapters and their authors are as follows: the nature of psychology (E. G. Boring); the response mechanism (C. T. Morgan); response (C. T. Morgan); growth and development (L. Carmichael); feeling and emotion (W. A. Hunt); motivation (D. W. MacKinnon); learning (C. Hovland); retention and transfer of learning (C. Hovland); recollecting, imagining and thinking (T. A. Ryan); perception (E. B. Newman); sensation and psychological measurement (S. S. Stevens); color (F. L. Dimmick); visual space perception (F. L. Dimmick); hearing (E. B. Newman); taste and smell (C. Pfaffmann); somesthesia (F. A. Geldard); topographical orientation (D. G. Griffin); individual differences (A. Anastasi); heredity and environment (A. Anastasi); efficiency (M. E. Bitterman and T. A. Ryan); personality (L. F. Shaffer); personal adjustment (L. F. Shaffer); vocational selection (C. L. Shartle); attitudes and opinions (H. Cantril); and social relations of the individual (L. P. Crespi).

This is an impressive list of highly competent authors assigned to write on the broad fundamentals of general and applied psychology. The sequence of topics conforms to widespread opinion, and the various presentations are in general clearly and authoritatively written. The chapter on topographical orientation is the only one whose presence is apt to be seriously questioned. The subject is interesting, as are many others not included in the book, and well organized; but to the reviewer the topic has no valid claim for inclusion as a major item in an introductory text. Were it not for the fact that the chapter on "Response" is already probably overloaded with a miscellaneous series of topics, one would be inclined to say that topographical orientation belonged in part there and in part under "Hearing" and "Visual Space Perception."

It fell to Boring's lot to write the first chapter on "The Nature of



Psychology," the most difficult one to write, the least satisfying one in the earlier editions, and the one of most questionable value in any beginner's text. Except in this first chapter, there is almost no reference to consciousness in the volume, other than the casual use of the term in such expressions as "conscious pangs of hunger" and "the stream of consciousness." Boring, however, tells the reader emphatically that consciousness is a part of the subject matter of psychology, the other part being behavior. The individual authors nevertheless do not seem to justify such a statement by their treatment of the science. Certainly the student will be at a loss to find discussions of consciousness in the body of the text to compare or to coordinate with the accounts of behavior. Boring's statement that "only a hundred years ago you would have been told that psychology *is* the study of consciousness" (p. 5) will also seriously confuse the student unless he notes (p. 9) that "As late as 1910 . . . most psychologists . . . believed that physiology studied the body, that psychology studied consciousness and that introspection was the direct way of getting at consciousness." The place of consciousness in the psychology of the first chapter is further confused, for the reviewer and perhaps for professional readers, by the statement that "A science of psychology cannot, of course, be built up on any one man's experience, so most of consciousness for every psychologist is the experience of others." Presumably this experience of others is accessible to scientific study. In the discussion illustrating the nature of consciousness, the author includes the following statement: "One of the commonest illusions is seeing a single object as single with two eyes." If the object is single, seeing it as single should not be an illusion in the general acceptance of that term, although seeing it double might be. Ordinarily the term illusion is applied where there is a discrepancy between the "experience" and the "real object" or between the way in which the subject behaves in the experimental situation and in a standard situation where rulers, compasses, or tactual verifications can be employed. Perhaps, however, even for the author, consciousness is a scientifically negligible affair since he includes no definitions of such processes in the list of terms at the close of the chapter.

There are two additional points in this first chapter where adverse criticism seems to be in order: (1) Figure 2 purports to show the chronological and subject-matter relationships between the schools of psychology. It would take an undue amount of space to query this figure in detail. The only clear and unambiguous features, to the reviewer, are the dates. Surely then no beginner could interpret it and come out with anything approximating the historical relationships. (2) Under the heading of experiment as a part of scientific method, the author states that "Experiment is the observation of *concomitant variation* and the interpretation of the concomitances as causes and effects." Now the use of concomitant variations is an extremely important method of

experimentation, but it is not the only one nor is the interpretation of "concomitances as causes and effects" as wise and safe a procedure as the statement implies. To interpret correlation coefficients or learning curves as cause and effect relations between the variables concerned can result in some strange conclusions. The experimental methods of agreement and difference, to use Mill's terms, deserve more specific recognition than they get under the author's term "control." Where one deals with the determination of the effect of benzedrine on intelligence scores or with the effect of the extirpation of one brain area upon behavior, it is confusing to try to fit the methodology to concomitant variation as though one must always deal with continuous functions.

Although the authors of the various chapters present highly competent and sometimes brilliant expositions of their topics, it is always possible for a reviewer to indicate ways in which the accounts suffer or ways in which they could be improved from his point of view. In the spirit of this observation, the following points are selected for mention: (1) In spite of the amount of space allotted to the nervous system and the receptors, no indication is given of the afferent nerves involved and but little mention is made of the effects of nerve lesions. (2) Neither in the chapter on psychological measurement nor in the one on individual differences can the student learn the simple statistical formulae necessary for his work or how to use them. (3) The chapter on color might have given less space to color equations and the color pyramid and more space to such psychophysiological topics as dark adaptation, integration, intensity-time relationships, etc. The section on "physiology of color vision" is notably weak. (4) Stevens, in the chapter on sensation and psychological measurement, writes as follows: "The sense organs start the messages along the nerves, the highways to the brain. When these messages merge at the higher centers of the nervous system, when they organize themselves and modify one another through interactions and associations, we call the result perception. But the bare messages themselves, isolated and apart from their mutual influences, we call sensations. We get sensations by analysis, by paying attention to certain aspects of our perceptions. . . . By selective attention he [the artist] analyzes the organized scene into patches of color and he translates these sensations into pigments on canvas." Now the reader has already (painfully) learned, in the chapter on the response mechanism, that the messages carried by the nerves are electrical changes. As Stevens states it, electrical changes are therefore the sensations, for example patches of color; and we get at these electrical changes by paying attention to aspects of perception which are the integrated electrical changes in the brain. The reviewer does not believe that Stevens means what he says; but he believes that the careful student will be confused by the account.

In the editorial preface to the volume, it is pointed out that the present book is about twice as large as the previous edition. "The strong-

est evidence of the rapid advance of psychology is the need of frequent revisions of textbooks. It is nine years since our last book, *Introduction to Psychology*, was published. In the meantime we have had a long war and a victory which psychological research helped attain. In this war-time research much new and valuable knowledge came into being. In addition there has been the more normal acquisition of facts as well as a clearly distinguishable change in point of view." Here we deal with matters of opinion. The reviewer finds no new point of view in the book and no chapters on topics which could not have been well discussed ten years ago. The reviewer does not believe that the advances made by psychology in the past nine years have been so extensive as to require a doubling of the size of elementary textbooks. Nor does he believe that wartime research produced more than a barely appreciable amount of new knowledge in psychology, although the applications of existing knowledge were varied and extensive. During the war, as before and after it, the fundamental handicap of applied psychology was the limited amount of basic knowledge available from general psychology. Such knowledge comes slowly. It is the reviewer's guess that the increased size of the present textbook over its predecessors is due far less to the growth of psychology than it is to the combination of changed contributors and the probable instructions given them on the lengths of their respective contributions.

In its current size, the justly famous BLW text steps out of the class of one-semester books and into the class of texts for full-year courses. Whether or not this is a pedagogical gain remains to be seen. However, to the student or the young instructor who masters the volume, there will come an excellent perspective view of the science which will aid him to keep his thinking on a straight course through the confusing eddies and back waters of psychological activities in the post-war period.

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DENNIS, WAYNE (ED.). *Readings in the history of psychology*. New York: Appleton-Century-Crofts, 1948. Pp. xi+587.

Should the history of psychology be taught from original or secondary sources? If the student is to learn by eye, from the printed word, he will have to use secondary sources. The instructor could not provide fifty students with access to the original writings of, say, one hundred men. The books of the very great may be available in reprints and translations but not so often their crucial articles, except when the papers have been collected in anthologies. Dennis' anthology of sixty-one historically significant excerpts from forty-nine authors does us, therefore, a service. It enables the teacher to supplement one of the standard texts by samples of original sources. In this way many stu-

dents will come better to comprehend the temper of some of the famous psychologists and the atmosphere in which they wrote.

Rand's *Classical Psychologists* of 1912 had a similar use, but it is out of date. History gets out of date. In 1912 the philosophers were much more important and the physiologists much less important than in 1948. Besides, 1948's past can now contain a little of what was only 1912's future.

Dennis does not, however, give enough material to make his book satisfactory as a sole text. It is conceivable that a thousand pages consisting of one hundred excerpts from eighty authors might do as a sole text, with the instructor's lectures filling in the interstices and setting up the dynamic interrelations, but I am not sure. Direct quotation is so uneconomical. What had to be labored in 1838 can be quickly understood in 1948. Johannes Müller's elaboration of his ten laws of specific nerve energies was necessary then, while thought still resisted his dicta, but not now, when what he was saying has become a commonplace.

The simplest way to indicate the content of this book is to give a chronological table of the forty-nine authors and their sixty-one excerpts. Here each mention of an author indicates a single excerpt, except when a parenthetical number indicates a joint authorship or more than one excerpt from the same author in the same period. In most cases readers will be able to guess the nature of the subject matter from the date and the author.

Antiquity: Aristotle (2).....	2
1601-1650: Galileo, Descartes.....	2
1651-1700: Hobbes, Mariotte, Newton, Locke.....	4
1701-1750: Berkeley, Hartley.....	2
1751-1800: Mesmer, Thos. Young, Dalton.....	3
1801-1810: Thos. Young.....	1
1811-1820: Chas. Bell, Thos. Brown.....	2
1821-1830: Flourens, Jas. Mill.....	2
1831-1840: E. H. Weber, Joh. Müller.....	2
1841-1850: J. S. Mill, Braid, E. H. Weber, Helmholtz.....	4
1851-1860: Helmholtz, Fechner.....	2
1861-1870: Helmholtz, Galton.....	2
1871-1880: Wundt.....	1
1881-1890: Preyer, G. S. Hall, Galton (2), James, Ebbinghaus, H. H. Donaldson, Cattell (4).....	11
1891-1900: Dewey, Titchener, Thorndike.....	3
1901-1910: Thorndike (1½), Woodworth (½), Binet, Simon, Pavlov, J. R. Angell.....	6
1911-1920: Watson, Hunter, Cannon, Terman, Köhler (2), Franz (½), Lashley (1½).....	7
1921-1930: Yerkes, Carmichael, Barbara Burks, Lashley, Hull... ..	5



It is plain that Dennis has welcomed the philosophers with acute discretion, admitted the important physicists, turned definitely toward the physiologists, and accepted mental testing, while he excludes both abnormal and social psychology. Galton, Cattell as tester, Binet and Terman are here, but there are no signs of Charcot, Janet, Freud and the contemporary psychopathologists, nor of Le Bon, Tarde, McDougall and the modern American sociotropes. In this Dennis follows more than he leads, for this history which he documents is that part of psychology's history which is best known and has been most fully presented in texts.

The distribution against dates seems to me to be about right. Aristotle should come in, but the rest can be omitted until the seventeenth century. Rand gives fifteen philosophers before Hobbes to Dennis' one. It is jolly to have at hand that passage from Galileo on pitch and frequency. At the other extreme of time I pause to consider and assess my contemporaries. Dennis cites twelve living men, all except Köhler born in America. Dewey at eighty-nine is the oldest, Carmichael at fifty the youngest. The baby is Barbara Burks, who would have been forty-six this year. I doubt if she should have been included (the only woman in the list, too), but the others all seem to me to be justified examples of important current history. Discrimination in this matter is, of course, as invidious as it is necessary, but I am ready to support Dennis. Of the class of living APA ex-presidents over fifty years old, Dennis has included nine and omitted twenty-one. If we accept his exclusion of abnormal and social psychology from the project, then I think Dennis has done pretty well. I should add to his list, considering their contributions quite as important as Carmichael's on maturation, only these two: Tolman on reaction as meaning or on purposive behavior (as he takes the torch from E. B. Holt whom I should also want to work in), and Thurstone on mental abilities.

Now what we have here is sixty-one samples of historically significant contributions by forty-nine psychologists. If I were free to enlarge this book, I should keep all of these samples with five possible exceptions. (1) I should leave out J. S. Mill on ethology and substitute Bain on laws of association, putting in Mill on the nature of matter, which is his theory of perception. I think it had more influence in the thought of that time. (2) Wundt is indeed a puzzle. What should one quote? These three pages on psychological methods from the fifth edition of the *Physiologische Psychologie* (1902) are quite inadequate. You get a fuller discussion of this sort in the introduction to his *Sinneswahrnehmung* (1862), where Wundt might be said to be 'founding' the new psychology, and you get something more mature in his *Ueber psychologische Methoden*, the leading article to the first volume of the *Philosophische Studien* (1883). Search ought to reveal something suitable on *Elementen* and their *Verbindungen*, the crucial topic in Wundt's system.

Perhaps some of *Physiol. Psychol.*, 1902, I, 339-350 would do, if nothing better turns up. (3) The only reason for quoting H. H. Donaldson on the temperature spots is that he is already in English. Blix or Goldscheider would be more appropriate, but I should quote von Frey who brought this skin theory together and defended it. If the articles in the 1890's do not yield suitable sections, then his *Vorlesungen über Physiologie* (1904) will. (4) Cattell has three papers from Wundt's laboratory (1885-1887) cited, besides the famous one on mental tests. It is not fair to the Leipzig laboratory to have Cattell its sole representative. Wundt's remark that Cattell was "ganz Amerikanisch" might be extended to include the functional orientation of Cattell's research. Nor does Cattell merit more quotation than any other author. I should keep one of his three, and add from Wundt's *aegis* two others which have special historical significance—perhaps L. Lange on reaction (beginning the experimental psychology of attitude, 1888) and Meumann on the time-sense (1892). (5) And then I should omit Burks on nature-nurture because I do not think her paper had great influence.

Those five changes would reduce the sixty-one items to sixty. Now here are forty-five more that seem to me to be of an historical importance as great as the items already included. Besides enlarging the book, they would also fill in lacunae and provide greater dynamic continuity for the genetic process being sampled.

Let me begin with Germany and Austria. It would be interesting to have something from the astronomer Bessel on the personal equation (1823). Purkinje's account of the phenomenon that bears his name (1825) is short, and the student will enjoy being shown that Purkinje observed 'twilight vision' at dawn. It would be well to have something from Herbart (1824) about the limen of consciousness and the inhibition of ideas. Then there would be Lotze and I should hope the *Medizinische Psychologie* (1852) would yield a section on local signs. Hering ought to come in somehow. Perhaps his nativism (1864) is more significant nowadays than his color theory (1874). Next I should hunt for a suitable account of brain localization in Fritsch and Hitzig (1870) and for proper comment by Brentano on the nature of act and intention (1874). Mach ought not to be left out for he leads straight into Gestalt psychology. His *Analyse der Empfindungen* (1886) would provide a section about the positivistic answer to dualism or on space sensations. There would hardly be room for both. The best thing to take from Stumpf's *Tonpsychologie* would be the description of tonal fusion (1890), but there is also his significant adoption of the psychology of function in 1907. We might have both of them. In the wordy, polemical G. E. Müller it is hard to find brief quotable material, but what about his psychophysical axioms and his hypothesizing of cortical gray (both 1896 and related)? I have already suggested Wundt on elements and their combinations, but perhaps we should also have Wundt on reaction times, since mental chronometry characterized the Leipzig laboratory. It will not do to omit Külpe entirely, and his 1892 criticism of the subtractive procedure in reaction times (especially if Ludwig Lange is included) would do for the early Külpe, and his 1912 paper on thought for the later Külpe. In the Würzburg School, we might

hope for Watt on fractionation (1904) and Ach on determining tendency (1905). To round out Gestalt psychology, we ought to have, I think, von Ehrenfels on form-quality (1890), something from Schumann on perception in his papers of 1900-1904 (the Gestalt psychologists do not accept Schumann in their ancestry, but I think he is significant), and certainly an excerpt from Wertheimer, for which I nominate a selection from the 1921 paper in the *Psychologische Forschung*. That leaves out of my list Loeb on tropisms (1890, or better 1899) and Lipps on empathy (1903).

With abnormal psychology ruled out, the French become less important. There are already six French articles included. Gall on phrenology (ca. 1825) and Broca on the speech center (1861) might be added. It does not seem right to include Binet and exclude Ribot, but so many of Ribot's books were on mental disease. Could his *Psychologie de l'attention* (1889) furnish a significant section? And then what about Piéron? Does not France provide one living psychologist to rank with America's twelve?

My recommendations for Great Britain are these. Robert Whytt on the reflex (1751). Marshall Hall on the reflex (1832 or 1837). Thomas Reid on the powers of the mind (1785) as faculty psychology and leading into phrenology and factor analysis. Bain on association (1855), in addition to James Mill and instead of J. S. Mill on this topic. Spencer on evolutionary psychology (1870-1872) if the right passages can be found. James used Spencer's *Principles* as the only text in his course in physiological psychology. Ward on the subject-object relation, cognition and conation, from the 1886 *Britannica* article or the 1918 text. Romanes (1883) to show how careful he was about the anecdotal method. Lloyd Morgan (1894) for his canon. Spearman on *g* (1904). C. S. Myers ought to appear, but I do not know of anything truly significant for his role in British experimental psychology. Perhaps we must leave him out. Godfrey Thomson can certainly be included on overlapping abilities. I think of the 1916 paper in the *Brit. J. Psychol.*, because it had influence. And then there is McDougall on instincts (1908) and on purposive behavior (1923). Do we put Bartlett in? On reminiscence perhaps?

Now the Americans. There are none before Stanley Hall (1883) in Dennis' list. From Hall onward eighteen of the twenty-five psychologists are Americans. Since 1900 there are thirteen papers by native Americans and only five others. Perhaps Dennis has already looked after America well enough. I do, however, have some additions to suggest. James ought not to be limited to his theory of emotion but to have something that shows his position vis-à-vis functional psychology and Gestalt psychology. That can come from the *Principles* (1890). J. M. Baldwin should appear on evolutionary psychology but I do not know what to quote (1895 or 1897). Münsterberg should come in on applied psychology. Titchener, in addition to the paper on structural psychology, might be quoted on introspection (1910) and the nature of feeling (1908) or of thought (1909). Jennings on consciousness in protozoa (1904) is needed to offset the tropisticists. E. B. Holt on consciousness as reaction (1915) ought to be in, because he leads into Tolman who leads into operationism and modern behavioristics. Yerkes is now in on the Army intelligence tests, but I should like also to see him standing by his apes. Tolman should somehow receive the ball from Holt. And Thurstone should be there as the terminus of the series Reid, Gall, Galton, Cattell, Thorndike, Binet, Spearman, Terman, Thomson.

The net result of all this discussion is the suggestion to delete four of Dennis' sixty-one papers and to add forty-eight new ones, making a new total of 105.

Now let us see how this is related to countries and languages. We shall count contributions, not men.

	<i>Germany and Austria</i>	<i>Great Britain</i>	<i>America</i>	<i>France</i>	<i>Greece</i>	<i>Italy</i>	<i>Russia</i>	<i>Total</i>
<i>Dennis</i>	12	16	23	6	2	1	1	61
<i>Dennis and Boring</i>	34	29	28	10	2	1	1	105

Of the twenty-nine from Great Britain, nineteen were prior to 1883, which is the date of the earliest American paper. The associationists, the physicists and the biologists of Great Britain contributed most heavily to the early preparation for modern psychology.

The ratio 34/28 would not seem to me to weight the German papers too heavily in relation to the American. The ratio would be still larger if you stressed the nineteenth century more heavily in relation to the twentieth. Somewhere in the nineteenth century the Germans took over from the British, somewhere in the twentieth before Hitler the Americans passed the Germans.

Thus we arrive at the problem of translations. To be used as a text in American universities, such a book must be printed entirely in English. Dennis got along with the minimum of translation, for, of the twenty-two contributions in five foreign languages, nineteen were already printed elsewhere in English translation. The two pages of Mariotte on the blind spot and the eleven pages of Flourens on the nervous system were specially translated from the French for this book. Two pages of Helmholtz on the rate of the nerve impulse were translated specially from the German. How much new translating would be required if my proposal for a total of 105 contributions were to be put into effect? Twenty-four papers, twenty in German and four in French, with the possibility that two German translations could be saved if you took the less satisfactory but already translated books of Herbart and Lotze. That is a very considerable undertaking. It might run to three hundred pages of new translation and it is no wonder that Dennis did not undertake it.

Has not Dennis perhaps here made an answer to the contention of the Minnesota psychologists that the young new psychologists do not need to spend their time learning French and German because "in the few instances where a graduate student needs to consult a foreign language publication, it would be more economical of the student's time and



energy for him to have the material translated for him" (*Amer. Psychologist*, 1947, 2, p. 136)? If it were so simple as all that, Dennis could use the facilities which graduate students use at Minnesota and include my twenty-five German and French papers in English translation in his second edition. Certainly if we are to get those translations, Dennis must get the work done for us. When he has passed on, his successor may not know enough German to decide what to translate. You do not simply send the 53,735 German pages which Wundt wrote down to the university's translating bureau for an 'English' of them, so that you can then decide which are important enough to use.

About this useful book of Dennis' I have one minor bitterness, directed more at the publisher than at the editor. The right-hand page heading shows the topic being considered. The left repeats 'endlessly' (266 times) the title of the book. No chance that the reader will forget what book he is reading, and also but little chance that he can find what he is looking for by leafing through to it. I tested myself out guessing authors from the sixty-one page headings and missed on twenty. "Power of the Mind": that ought to be Reid, ought it not?—or Dugald Stewart, or maybe Gall. It *is* James Braid, of all persons.

All in all the book is a good job and Dennis earns our gratitude. The courses in the history of psychology will have an additional text, and the browsing graduate student with scholarly inclinations will often be found reading it.

EDWIN G. BORING.

*Harvard University.*

LEWIS, D. *Quantitative methods in psychology*. Iowa City: Don Lewis, 1948. Pp. 286. (Lithoprinted.)

This is the first book to present an account of quantitative procedures for psychologists at a more basic level than the usual treatise on statistics. In *Quantitative Methods* Lewis provides material for training the psychology student in basic mathematical techniques and gives him opportunity, in appropriate exercises, to apply the techniques to problems of psychological interest. If it happens, as a result of the approach exemplified by Lewis, that quantitative psychological theory is made meaningful to a large number of students and eventually becomes a matter of interest and practice to all well trained psychologists, then *Quantitative Methods*, as one of the first formal symbols of such a state of affairs, may have an important place in the development of psychological science.

Chapter 1 deals with some elementary principles and Chapter 2 considers the fitting of straight lines to empirical data. Chapter 3 is on logarithms. All of these chapters are straightforward and clear.

The use of non-linear curves is treated in Chapter 4. The chapter does not give an exhaustive account of the great variety of curves

available for representing data, but the material covered is presented in a useful way. In only one respect do the reviewers disagree with the treatment of curve fitting: the application of a special case of the method of least squares to all forms of data. It is obvious that the method of least squares is one which is based upon certain assumptions, and if the assumptions are not fulfilled by the data, then the results of using the method are uninterpretable. When Lewis fits curves other than straight lines to data, he achieves the fit by transforming the data to yield a straight line and fitting this transformed function by least squares. Such a solution involves many assumptions, some of which are mentioned, none of which is emphasized. The problem of what deviations from best fit to minimize is always an important problem. If the data warrant elaborate treatment, the rationale of the steps used in fitting a curve to the data may be developed and may involve a complex analysis. If the data do not warrant such treatment, the fit may be chosen more obviously on a basis of convenience. In the latter case, some of the other methods mentioned by Lewis will yield easier and equally fruitful solutions. Considerations respecting least squares also apply to the discussion of the index of determination (p. 81).

Chapters 5 and 6 present a short account of calculus. Differentiation is treated in Chapter 5 and integration in Chapter 6. The treatment of calculus is probably less extensive than may be desirable. The discussions of differentiation, integration, and partial differentiation are condensed, and there is no consideration of methods of integration or of the use of integration tables. The problems of repeated integration and elementary differential equations are not mentioned.

Most of the latter part of the book (Chapters 7, 8, 10) is concerned with problems of the normal curve and problems of goodness of fit. Chapter 7, on the normal curve, is good and Chapter 10, on the fitting of straight lines for testing the significance of parameters is also valuable.

Some portions of the treatment of distribution functions (Chapter 8) and goodness of fit (Chapter 10) will be much less obvious to students than the discussion of earlier topics, such as logarithms and differentiation, and the use of the book as a text may require considerable supplementary lecturing. There may be some question, for example, as to the value of presenting the formula for the chi-square distribution (p. 174) without showing how one arrives at it; Lewis uses the formula to arrive at the mean and modal values of such a distribution. Such a procedure may help to eliminate some fear of large formulae but, from the point of view of understanding the distribution, the student might just as well be given the mean and modal values arbitrarily.

Chapter 9 is one of the most valuable chapters in the book. It gives the student an opportunity to apply the principles he has learned to a connected background of theory in his own field. The treatment of Hull's theory (p. 233 ff.) is good, and the description of Hecht's photo-

receptor hypothesis is valuable, although presentation of a later version of the theory would be desirable. Finally the reviewers believe that Woodrow's equation (pp. 247 ff.) receives more extensive treatment than it merits.

One is tempted to compare *Quantitative Methods* with books in other fields written to serve comparable purposes. In physics, chemistry, and engineering, one encounters treatises on quantitative methodology which range in difficulty from Daniels' *Mathematical Preparation for Physical Chemistry* to Mellor's *Higher Mathematics for Students of Chemistry and Physics* and the Sokolnikoffs' *Higher Mathematics for Engineers and Physicists*. Lewis' book lies in difficulty somewhere between the level represented by Daniels and levels represented by Mellor and the Sokolnikoffs. Despite the fact the Daniels book is written at a more elementary level than Lewis', it seems to the reviewers that the Daniels work is more satisfactory from the point of view of training in basic mathematical techniques, particularly in applications of calculus.

It may be argued that a course which is primarily of psychological interest should not treat topics such as calculus which can be learned in other courses. Nevertheless, it has been the experience of the reviewers that a course in quantitative treatment which emphasizes the application of calculus is of great benefit in elucidating the concrete problems in a student's own field.

A great deal of space in *Quantitative Methods* is taken up by statistical theory. This orientation is an important one and must not be underestimated. Nevertheless, it presents certain difficulties, not the least of which may be the development of a type of thinking which is more concerned with establishing differences than with describing functions. An additional danger lies in the fact that the student may become submerged in details of special statistical techniques without receiving an understanding of mathematical thinking. The reviewers agree with Lewis that psychological theory can benefit greatly by the application of mathematical thinking to many problems. However, they have a prejudice that basic mathematical techniques rather than statistical analysis should be emphasized in an orientation course on quantitative treatment. Training in advanced statistics may progress more effectively in later courses against a background of well learned mathematical operations.

Despite our opinion that Lewis's account should be strengthened in its treatment of basic topics, the book has much to recommend it. Let there be no question about the attitude of the reviewers: Lewis has written a valuable book. It is the first book to present an appreciative account of materials requisite to the quantitative thinking of psychologists, and, for this reason, it should receive wide attention and influence the graduate training of psychologists in a desirable way. If the

book stimulates the mathematical training of psychologists, it will be a major contribution, and other books dealing with the same topic may be expected to follow. If these books improve on Lewis', it is likely they will do so by emphasizing the rationale of analysis rather than the techniques. In any case, *Quantitative Methods* evidences an appreciation of an important gap in the training of psychologists and fills the gap with no little success.

C. H. GRAHAM.  
C. G. MUELLER.

*Columbia University.*

RUCH, FLOYD L. *Psychology and life*. (3rd Ed.) New York: Scott, Foresman, 1948. Pp. 740.

Ruch's new edition of *Psychology and Life* is an enlarged, better written and illustrated version of his earlier volumes.

In evaluating such a textbook one must of course acknowledge that the merit of a particular book depends upon the instructor's interests, bias, and philosophy of the purpose of psychology. Relative merit also depends upon the level of ability of the student, the major course the student is pursuing, and the probability of his taking further psychology courses.

If the instructor wishes to emphasize the physiological-experimental-scientific approach, Ruch's third edition probably falls short of Munn's *Psychology*. If the instructor wishes to remain in the traditional frame of perception, sensation, learning, memory, thinking, but with perhaps a bow to the application of psychology to life, the *Psychology* of Woodworth and Marquis is probably more suitable than *Psychology and Life*.

However, for the instructor whose students will have little further work in psychology and who expect some immediate answers to everyday problems, Ruch's revision has no competitor. Whereas, his former editions have been overly talkative in parts, have admittedly stressed subjects "interesting" to the student, have been weak in systematic presentation, the present edition has none of these weaknesses. Statements are well documented. The writing is direct, easier to understand, better organized. In the trend of picturization so well done by Munn, Ruch has added a variety of excellent drawings, some in color, and a number of well chosen photographs.

Considerable material has been added by reducing the size of print and tightening the exposition. Several chapters are now the result of collaboration with others: two chapters on learning (with Robert Leeper); "Emotional Development: Clinical Findings" (with Joseph Shoben); and "Illustrated Reference Manual: The Brain and Nervous System" (T. C. Ruch). Several editorial consultants are acknowledged.

A section on emotional adjustment includes some 135 pages, and is a distinct contribution for the student who is looking to psychology to



give him some insight into his own and others' behavior. Techniques of psychotherapy such as non-directive counseling, finger-painting, play-therapy, and psychodrama are presented in an interesting and basic manner. A new chapter on "Problems in Human Relations" which discusses marriage, parenthood and delinquency is characteristic of the movement into the applied area.

As to adverse criticisms, the revision will appear to many to but emphasize an already undesirable "popularizing" of psychology. It can be said for example, that it is unsound to introduce the topic of marriage in a beginning course in psychology, that marriage is the province of sociology, not psychology. Others who may accept marriage as a field for psychology will affirm that nothing good can come from a seven-page treatment. Many will be querulous about including such paragraphs as "What About Dunninger?" and "Is there Extra Sensory Perception?" (Students ask these questions even if the textbook writer does not.) Again, as in previous editions, the good rats do not run the students through mazes, although more attention is given to them than previously. This will no doubt continue to be construed by a few as an undesirable movement toward understanding people.

In fine, Ruch has continued to popularize his writing—but most of the previous criticism concerning looseness, lack of fact, and lack of a basic approach, have been met. Ruch has played to his audience, as any good teacher must—and why not?

The third edition of *Psychology and Life* is a real contribution to understanding life through psychology.

DONALD E. LUNDBERG.

Cornell University.

DAVIS, ROBERT A. *Educational psychology*. New York: McGraw-Hill, 1948. Pp. x+349.

This book is divided into two main parts. The first, entitled "Understanding the Learner," includes chapters on "Physical Growth and Health," "Scholastic Ability," "Interests," "Attitudes," and "Emotional and Social Maturity." The second, entitled "Directing Learning," takes up in successive chapters "Preparing Materials for Learning," "Cultivating Abilities," "Testing for Learning," "The Gains of Learning," "Maintaining the Gains of Learning," "Favorable Conditions for Learning," and "Incentives."

In an informative preface the author states that the book was planned for use in introductory courses in educational psychology and was intended to provide information concerning important problems of the classroom and the school, problems which teachers meet in their day-to-day work. Although psychologists will inevitably differ to some extent regarding the subject matter that teachers will find most useful, I think most psychologists will agree that the content of this book has

been well selected. What is equally important, I think there will be wide agreement that the book is unusually well written. The author's style is lucid and smooth.

The unique feature of his book is that the traditional subdivisions of educational psychology have been almost entirely ignored. One will look in vain for, say, a discussion of the law of effect or of Gestalt psychology. For this reason, the book may not appeal to some who organize and teach their courses in educational psychology in the traditional pattern. It seems to me, however, that good reasons can be adduced for reorganizing beginning courses in educational psychology to make them more functional, more interesting, and more nearly up-to-date. Dr. Davis has provided not only an interesting and well written text but a stimulus to further experimentation in presenting the subject matter of educational psychology.

FREDERICK B. DAVIS.

*George Peabody College for Teachers.*

GHISELLI, E. E., AND BROWN, C. W. *Personnel and industrial psychology*. New York: McGraw-Hill, 1948. Pp. xiii + 475.

As a survey of the major applications of psychological techniques to personnel problems in business and industry, this book is designed to meet the needs of university students, as well as those of persons in business and industry who desire to obtain a foundation in the basic principles of industrial psychology. The book represents a culmination of four earlier mimeographed editions, which were used in classes at the University of California over a period of several years.

The sixteen chapters are arranged in a logical sequence. Following the introductory chapter, in which emphasis is placed upon the scientific method as applied to *human* (psychological) as contrasted with *mechanical* (physical) factors in production, the topics of job and worker analyses, criteria of job proficiency, and rating methods are successively treated. An understanding of these techniques is basic to all of the topics subsequently presented. The next chapter deals with the interview and personal-data analysis, and is followed by three chapters dealing with various aspects of the validation and use of psychological tests. Subsequent chapters treat the definition and meaning of effective work (including efficiency, fatigue, etc.), conditions of work and productivity (diurnal changes, length of work period, rest pauses, shift work, physical conditions of the work environment), effective methods of work and the design of tools and equipment (time and motion study, work simplification, layout, and what has recently come to be called "bio-mechanics"), industrial training (two chapters), accidents and safety, monotony, and worker motivation and morale. The volume contains 115 tables and 38 figures (all line drawings), selected references at the end of each chapter, and terminal indexes by author and subject.

The general format of the book is identical to that of other volumes in the McGraw-Hill series, under the Consulting Editorship of Dashiell.

In the reviewer's opinion, this book provides an excellent presentation of the basic facts and methods of industrial psychology, and represents the best textbook currently available in this field. The presentation is sound and lucid, and there is repeated emphasis upon the need for empirical validation of techniques. Although classical material is well represented, frequent reference is made to recent experimental data, such as those obtained in World War II. The *limitations* as well as the *possibilities* of various techniques are clearly indicated. Thus, for example, in the discussion of the limitations of personality questionnaires it is excellently stated that the validity of such a questionnaire "will be a direct function of the honesty and accuracy with which the individual answers it" (p. 169). Hence the reader is prepared for the conclusion that personality inventories are far more valid in guidance or counseling than in selection situations.

Despite its many virtues, however, this book shows a few unfortunate short-comings, many of which represent errors of *omission*. Certain topics, such as job evaluation, are not treated at all, although such an omission is, curiously enough, characteristic of most textbooks by psychologists in the industrial field. Nor is any reference made to market survey techniques—an omission which is less serious since the authors evidently regard consumer research as outside the field of industrial psychology. Another minor but important omission occurs repeatedly in the tabular material throughout the book: experimental data are often reported without indication of the size of the sample on which such data are based; nor does the accompanying text reveal the number of cases in such instances.

Incentive systems are also inadequately treated. Although rating methods and their pitfalls are thoroughly discussed, little is said regarding the importance of *training* the raters. The distinction between *production* and *productivity* is not clearly drawn (cf. e.g., pp. 12-13, 239)—a distinction which is currently recognized as of fundamental importance in the labor relations field. Throughout the book, furthermore, the authors emphasize the importance of applying scientific techniques to the *worker*, but the application of similar techniques to all levels of *management* is either ignored or under emphasized, despite their equal relevancy in both fields of application. Moreover, the authors' contention that "the facts point to the 'sore spots' of production lying with the worker" (p. 17) is somewhat at variance with the opinion of many consultants as well as with the authors' own statement that the key to "more effective work through better worker morale" is "more understanding supervision" (pp. 557-458). Certainly, most industrial consultants would emphasize the critical rôle of at least "middle management" (foremen and supervisors) in the current industrial picture.

In the introductory chapter and in certain other places throughout the book the authors state a theoretical point of view which may be misinterpreted by those psychologists who are quick to applaud any deprecation of objective and rigorous formulation of psychological principles. Thus, the authors state that "interest should be centered upon the worker as a thinking, feeling, and desiring organism, and *not* as a mechanical device that responds in given ways to appropriate stimuli" (p. 2). The intent of the authors would appear to be that of emphasizing the *error of a priori assumptions* regarding the worker's motivational stimulation, the importance of considering the *motivation and attitudes of the individual worker*, and the need for experimental investigation of such motivation and attitudes in *specific working situations*. Unfortunately, however, many readers with a mentalistic bias will misinterpret such a statement, and will construe it as a denunciation of the applicability of S-R concepts—or even of the scientific approach—to the industrial field! The writer feels certain that Ghiselli and Brown would not subscribe to such an obscurantistic viewpoint.

One further point deserves special mention. In reviewing the historical background of the applications of psychological and engineering techniques to production problems (cf. pp. 6-7, 14-16), the authors indict management—and indirectly the engineer and the industrial psychologist—for having in the past directed attention solely to the goal of increasing production and hence of increasing profits. It is pointed out, to be sure, that industrial psychologists have, in recent years, fortunately turned their attention to problems much broader than that of "production and profits." The authors fail to emphasize, however, that *the services of competent consulting psychologists are available to labor as well as to management*, and that *the failure of labor unions to utilize such services cannot be laid at the door of either management or the consulting psychologist*. Although this point is not made by the authors, there is a hint of it in a later section of the introductory chapter. In discussing the need for research under actual working, rather than artificial laboratory, conditions, the authors state that such an objective "would require that industrial and union organizations jointly provide the means for establishing research groups within the industrial organization" (p. 18).

Everything considered, however, the merits of this book clearly outweigh its shortcomings. It represents a real contribution to a field in which there are all too few good modern textbooks.

JOHN P. FOLEY, JR.

*Industrial Division, The Psychological Corporation.*

BURTT, HAROLD E. *Applied psychology*. New York: Prentice-Hall, 1948. Pp. x+821.

The advent of a new textbook whose author is a recognized pioneer in his field is always an interesting event. Most of us are engaged in



the never-ending task of finding a text which best suits our needs and efforts. More important, however, is the fact that such a book inevitably implies a definition, or at least a delineation, of a field. These definitions, more frequently than those of the more concise and formal type, may affect materially the subsequent course of work in the defined area. This is particularly true if, as in this case, the author writes for students and laymen (perhaps actual or potential employers of applied psychologists) alike.

It must be recognized, in short, that Dr. Burt's book will have considerable influence upon what psychology instructors, their students, and laymen think applied psychologists should do and how they should do it. Its evaluation in this respect, as well as a pedagogical instrument, therefore seems in order.

The scope of this volume is wide. Apparently, the author conceives of applied psychology as a field which includes all efforts made by psychologists or other individuals to help anyone, regardless of whether the principles or methods of scientific psychology are involved. After an introduction and a quick tour of the field of general psychology, there follows a series of chapters which inspire the reader with considerable respect for Dr. Burt's breadth of interest and mastery of a voluminous literature. Three chapters are devoted to the contribution of psychology to education, two to personal efficiency and vocational guidance, three to medicine, four to law and crime, two to employment, three to industrial efficiency and morale, four to advertising, and one to "outlying fields" which include "Life Enjoyment" (the arts, recreation, and athletics), "Communications" (public speaking, writing, drama, movies, radio, television, and the press), and "Social Progress" (public opinion, politics, religion, war, and peace).

This implied definition of applied psychology will probably be as disturbing to some readers as it is to this reviewer. These readers will wonder if perhaps the field does not need a re-definition which would make it unnecessary for the author of a textbook to include a treatise on the nervous system, psychiatry, the problems of social workers, or the practices of advertising men. They may believe that it is more important for the student of applied psychology to understand the basic logic and techniques which are applied in the various fields than it is that he be familiar with the reflex arc concept or the alleged effects of frontal lobotomy. They may even feel that it is time applied psychology *defined* its field rather than embracing whatever can be engulfed by it.

These readers will soon find that Dr. Burt is in disagreement with their position. His emphasis is upon results—so much so that methodology is given very short shrift. The consequence is a series of deficiencies and inconsistencies that will distress the trained reader but will probably (and unfortunately) go unrecognized by the neophyte.

For example, the problem of sampling, so basic and so deceptive to workers in the applied field, is apportioned one page and this, many



chapters after the techniques of evaluation of the reliability of differences and the coefficient of correlation have been described. The reader is told that a modest payment for the return of a questionnaire is desirable because (p. 657) "... as the sample becomes larger, it becomes more typical." Elsewhere, in accordance with something less than a great tradition, he is informed that statisticians usually "... hold out for a ratio of 3 before assuming significance" (p. 94).

The perpetuation of these and like notions of the logic of the statistical method is disturbing. Even more serious, perhaps, is the author's tendency to exhort the reader to apply certain general principles in rigid evaluation of some efforts in the applied field and subsequently to forget, omit, or evade these principles in his own evaluation of work on other problems.

The "pseudo-psychologists" come in for their usual drubbing because of their failure to subject their notions to empirical test. Those who use psychological tests in selection are enjoined to remember that they must first have demonstrated "empirical" rather than "face" validity. Not the name of a test but "Validity is the all-important consideration" (p. 68). However, the clinicians, psychoanalysts, or even the psychology-conscious, pupil-guiding teacher who "once the underlying causes have been ascertained . . . is in a position to apply remedies" (p. 113), are not required to test their hypotheses or validate their procedures. In vocational guidance certain tests, such as those for "dexterity and coordination," "Even without empirical validation . . . , if interpreted with caution, may help one decide between clerical and mechanical work" (p. 200). Projective tests or tests of "recklessness" or "Social Intelligence" are also given a clean bill of health without any indication that the all-important consideration of validity for a specified criterion might properly be applied to them. It seems to this reviewer that such inconsistency will always result until psychologists are willing to recognize that the criterion is an important concept in almost any psychological investigation. Like many other authors, Dr. Burr't gives practically no discussion of the criterion problem. Like them also, he invites the suspicion that he attacks pseudopsychologists because of their interests rather than their methods. Unfortunately, pseudopsychology is not confined to the fields of telepathy, graphology, or the like, and the reader should be told so.

This is to emphasize the defects in Dr. Burr't's book. It will not be used as a text by those who believe that applied psychology is based more on methods than results nor recommended by them to the layman. On the other hand, for those who desire a textbook which recounts the wide variety of fields in which psychologists may be interested and the things they and others have done about it, this is a fine solution. Dr. Burr't knows the literature and he writes with charm and force. Few students will find difficulty in reading and enjoying him.

Even the paucity of illustrative material (22 illustrations and 47 tables in 802 pages), old-fashioned as many devotees of applied psychology may claim, does not lessen this book's freshness of style and agility of transition.

S. RAINS WALLACE.

*Life Insurance Agency Management Association.*

OTIS, JAY L., AND LEUKART, RICHARD H. *Job evaluation: a basis for sound wage administration.* New York: Prentice-Hall, 1948. Pp. xv + 462. \$6.65.

Perhaps more than any other publication in the field of job evaluation, this book discusses job evaluation from the point of view of its underlying psychological implications. By drawing parallels between the various job rating processes used and the rating methods on which they are respectively predicated, the authors have demonstrated (though they actually avoid saying so) that the field of job evaluation is grist for the psychologist's mill. They also emphasize the often neglected point that a job rating system should be so set up that it provides for only those refinements in judgments that can reasonably be expected to be made with relative accuracy.

As far as the over-all content is concerned, the book embraces the usual topics, including the rationale for job evaluation, a description of the various systems used, and methodology for developing and implementing a job evaluation system. In at least three respects, however, this presentation excels.

In the first place, the authors have placed particular stress on the implications of a job evaluation program in an industrial or business organization, such as its serving as a basis for a sound salary and wage administration program, its potential use in collective bargaining, its important use as a management control device, and its potentialities in providing a basis for better employer-employee understanding.

Second, the entire book is characterized, by an atmosphere of impartiality and objectivity, as illustrated by the consistent presentation of both advantages and disadvantages of various systems, practices, and procedures, and by the judicious sprinkling of words of caution which might prevent the unwary soul from falling into any of the numerous pit-falls.

In the third place, the treatment of procedures for developing and implementing a job evaluation system is exceptionally adequate; this undoubtedly represents the most effective contribution of the book. This thorough treatment is typical of the several major steps, including the development of the system, the collection and processing of job information, the rating and verification of ratings of jobs, the development of the pay structure, and the classification of employees. Variations of each of several steps are included where appropriate. This care-

ful attention to detailed procedures has, in certain instances, resulted in repetition or redundancy, although this tendency has not unduly marred the effectiveness of the book.

The discussion of procedures is distinctly not of the ivory-tower variety, since recognition is constantly given to those practical situations which may preclude a close hewing to a given theoretical line. For example, the authors make no bones about the acceding to the interests of either management or labor on the inclusion or exclusion of any given factor, in order to keep peace in the family.

The most vulnerable spot in the book seems to be the acceptance of the point of view that the selection and weighting of factors must of necessity be made on the basis of the best "guesses" (the authors use the word "judgments") of those responsible for the development of the system, relying on the past experience of other companies as a guide. It is entirely feasible for a company to determine factually the relative importance of the factors that are used in its system; the statement that "there is no known method and no known criteria for determining the relative values in an objective manner" seems just a bit strong in the light of the fact that such relative values *have* been determined in specific situations.

As a contribution to clear thinking in the field of job evaluation the book is of material significance. With full awareness of psychologists' taboos about dealing with superlatives, it is the opinion of this reviewer that the book is the most adequate over-all manual on the philosophy and techniques of job evaluation that has yet appeared.

C. H. LAWSHE.

*Purdue University.*

KAPLEN, OSCAR J. (ED.) *Encyclopedia of vocational guidance*. New York: Philosophical Library, 1948. Vol. I, pp. xxi+722. Vol. II, pp. 700. \$18.50.

Three hundred authorities in eleven countries contributed articles to this *Encyclopedia*. Topics which are covered include: vocational guidance activities throughout the world; vocational guidance periodicals and organizations; the handicapped; problems of minority groups; aptitude patterns in the principal occupations; occupational trends in the United States; economic aspects of guidance; techniques of interviewing; evaluation of guidance procedures; administrative aspects of vocational guidance; descriptive articles on the principal psychological tests; and other subjects.

The editor states that this book "is encyclopedic only in a relative sense; it does not pretend to cover in great detail all phases of the guidance field. The topics included were chosen from a master list and many important subjects were omitted because of lack of space." The en-

cyclopedia is designed primarily for professional counselors who do not have large library resources.

Because economic aspects of vocational choice are so important, articles have been prepared on occupational trends, unemployment cost of preparation for the major professions, use of census data, and average earnings. No attempt has been made to present information on the training requirements, economic outlook, or general nature of individual occupations.

Topics are arranged alphabetically in formal encyclopedia style. For example, in the first 15 pages we have the following articles: "Accountancy, Aptitude For"; "Achievement Tests, General"; "Achievement Tests, Use of"; "Acrophobia"; "Alliance for Guidance of Rural Youth"; "American Council on Education Psychological Examination"; "Acting, Aptitude for."

It must be emphasized that these two volumes comprise reference material and are to be used for the purpose of acquiring pertinent information quickly. It should be used in the same manner that a counselor uses the *Dictionary of Occupational Titles*—when a need arises. For example, if a counselor is called on to assist a blind person or a hard of hearing individual or a member of a minority group, a quick reading of the pertinent topic beforehand will be of great value.

Many of the articles are descriptions of principal psychological tests. It is difficult to avoid comparing these presentations with the descriptions and critical evaluations found in Buros's *Mental Measurements Yearbook*. The discriminating counselor will, I am sure, find the *Mental Measurements Yearbook* a more helpful tool in deciding what tests to order or to use. Evidently, Dr. Kaplan has a conflict about including material on tests. An "Encyclopedia of Vocational Guidance" could not well omit such information; on the other hand, the *Mental Measurements Yearbook* had both description and critical comments. Therefore, the editor took a compromise stand:

The primary purpose of the test articles has been that of description, although critical evaluations also appear with regard to the principal categories. The discriminating reader is referred to the *Mental Measurements Yearbook* edited by O. K. Buros (p. vi).

If one has the *Mental Measurements Yearbook* in his library—everyone using tests should—the articles on tests in this Encyclopedia are of little value.

Articles of great merit are the critical evaluations of classes of tests, such as, "Art Tests, Critical Evaluation of the Principle" and "Clerical Aptitude Tests, Evaluation of." Many critical comments on specific tests will be found in these summaries. From the user's standpoint it means reading at least two articles to find complete information on a given test.

The physical make-up of the books causes annoyance. There is no indication that Vol. I contains topics A through L and that volume II starts with M. Within a volume it is difficult to find a given title because of the lack of any indexing. Appropriate letters on the corners of the pages or thumb-nail indexing like a dictionary would facilitate quick reference.

Despite the objections mentioned the Encyclopedia is a contribution to the field of guidance. Along with the *Dictionary of Occupational Titles* and Buross's *Mental Measurements Yearbook* it should have its place on the bookshelf of every vocational counselor.

STANLEY G. DULSKY.

*Chicago Psychological Institute.*

NAUMBURG, MARGARET. *Studies of "free" art expression of behavior problem children and adolescents as a means of diagnosis and therapy.* New York: Nervous and Mental Disease Monographs, 1947, No. 71. Pp. xi+225.

This monograph is a collection of six articles published by the author in various psychiatric journals and in a volume on *Child Psychiatry* during the period between April, 1944 and January, 1946. All six studies are part of a research project at the New York State Psychiatric Institute and Hospital and are devoted to "the investigation of the possible use of 'free' or spontaneous art expression as an aid in both diagnosis and therapy" (iii). Each study demonstrates the extent and nature of the fulfillment of the project's aim by giving a detailed analysis of one child's graphic productions, conversations and general behavior during his periodic art sessions with the author.

By means of numerous reproductions Miss Naumburg shows convincingly how impersonal routine tracing and copying, common among public school children, gradually is replaced by spontaneous expression of personally significant subject matter: "As soon as original art work is encouraged, instead of dependence on models and specific techniques, the focus of a patient's art activity is modified. He will begin to draw on his own inner resources and this will inevitably lead to some expression of the conflicts within the personality, which may reveal aspects of the pattern of his mental disease as well as the specific insecurities or traumatic experiences within the patient" (p. 50). By releasing these largely hidden forces the author feels that the art productions of her patients serve the dual purpose of aiding the psychiatrist in diagnosis and in therapy. Whereas in a setting of this type both functions are more or less interwoven, the former goal is served primarily by bringing to the fore hitherto un verbalized diagnostic material, thus corroborating, anticipating or broadening psychiatric findings. The therapeutic contribution is in form of catharsis, substitute satisfaction for sup-



pressed desires and strengthening of self-esteem through artistic accomplishments.

Although the author's main purpose is to demonstrate the value of free art expression in clinical practice, occasionally she reaches out beyond her clinical material by calling imaginative expression "a source of growth and sustenance as well as a language of communication in the life of every individual, whether he happens to be mentally disturbed or at peace with himself" (p. 89). In fact, in one place we find the far-reaching assertion: "In the East unstinting recognition has always been accorded to art as an expression of and not as an escape from reality; could the West bring itself to an acceptance of all forms of creative expression as a universal normal and integrating experience that is neither effeminate nor neurotic, our culture might again find ways to restore harmony and balance to the disequilibrium of the modern psyche" (p. 90). Whether or not we follow Miss Naumburg all the way in these contentions, it cannot be denied that creative activity has a cathartic as well as constructively fulfilling effect. Any efforts to encourage its broader use in such fields as education, recreation or therapy can therefore only be welcomed.

So much for the main theme of this monograph. The clinically interested reader will surely find it stimulating and worth looking into. Those who wish to try out the technique discussed in it will appreciate both the specific suggestions regarding the author's actual procedure of starting the process of "liberating through art" as well as illustrations of her use of verbalizations concurrent with the graphic expressions. A person with wider interest, however, might well experience a certain amount of disappointment after finishing this book. One could wish that some attempt had been made to realize the potentialities of this rich and suggestive material for an advancement of our understanding of general theoretical questions such as physiognomic quality in perception, the psychological nature of symbolism, etc. Particularly the last study in the volume should lend itself well to theoretical exploration. It should be remembered, however, that the aim of Miss Naumburg's study was clearly stated as practical and not theoretical in nature and we all know how difficult it is to do equal justice to these frequently divergent aims within the framework of the same research project. The theoretical potentialities of this material are primarily suggested, therefore, as a realm for further expansion.

Finally it is regrettable that this monograph bears evidence of hasty preparation for the press. The articles, which had already been published separately, have been here thrown together into book form without any attempt at unification or organization except for a brief preface by the author and a foreword by Nolan Lewis. The same or very similar generalizations are repeated in many places. There is even one instance where a long passage from one article is quoted verbatim in another,

when a mere page reference would have sufficed. In addition to this repetitiousness among the separate articles, lengthy descriptions of the reproduced pictures appear in practically identical form in both the text and the caption of the picture. In short, greater editorial thoroughness would have much enhanced the readability of this worthwhile report.

MARIA A. RICKERS-OVSIANKINA.

*Wheaton College.*

LINNDER, ROBERT M., AND SELIGER, ROBERT V. (Eds.) *Handbook of correctional psychology*. New York: Philosophical Library, 1947. Pp. 691.

The kindest thing that can be said about this volume is that is a pioneering venture in an area that has been very inadequately explored. Although the authors titled the book "correctional psychology," and the inference is that by "correctional" is meant therapy in the penal sense, an analysis of the articles reveals less than one-quarter would not fit primarily into pure medicine, physiology in its applied phases, or sociology, especially in its forensic aspects.

For example, the reviewer was surprised to find chapters such as: "Electroencephalography: used in penologic practice"; "Sugar metabolism in its relations to criminology"; "The intramural practice of eye, ear, nose and throat"; "The acute medical patient"; "A venereal disease program in prison"; "Use of sedative measures in mental hospitals"; "Treatment of traffic offenders." While each and every one of these and other chapters can be argued for in a volume on criminal psychology, to say that the sum of articles such as these are "psychology" is to stretch the semantics a bit!

The bug-a-boo of all editors of compilations—variations in style and quality is well evident here. Some articles, like Brancale's "The classification clinic in a correctional institution" and Selling's "The extra-institutional treatment of sex offenders" are excellent, appear to belong logically within the area of discussion, and make the book valuable for those in the field, while other articles seem either so peurile, forced, or down-right silly that they make the reader wonder why they are included in a serious volume apparently designed for the use of specialists.

To whom is the volume addressed? It is too specialized to be used as a text in criminology; it is too superficial for the use of specialists in any area in penology; it is the source of too many original articles to be passed by. It is probably best suitable for a text for people in in-service courses in prisons who need to know the functions and problems of associated disciplines.

The need for a book that will discuss the origins, dynamics, therapies for criminal behavior has not been met. The present volume is not well-

balanced, but then neither is the field of penology these days. The fact that the editors were unable to find a single member of the American Psychological Association who was employed in the penal area to enter into this collaboration of 46 minds is a sad commentary on the present state of penal psychology.

The atrocious technical aspects of this volume deserve comment. The very first word of the book is mis-spelled—Editor's instead of Editors'. We learn there is a test known as "Revised Standard—Binet." In the Table of "contents" the titles of the chapters are not linked with the names of the authors. Some chapters have bibliographies, some do not. One bibliography is about 300 pages out of place. There is no index. The articles are not sectioned off in any logical manner. For the price, a more substantial binding would have been *en regle*.

In sum, it would be fair to say that there are various chapters that are well written and are unique and for these chapters the book is well worthwhile; and that while the assembly job seems sloppy that there is enough gold in the volume to recommend it for the person interested in correctional psychology.

R. J. CORSINI.

*San Quentin.*

COTTRELL, LEONARD S., JR., AND EBERHART, SYLVIA. *American opinion on world affairs in the atomic age*. Princeton: Princeton Univ. Press, 1948. Pp. xxi+152.

One of the greatest scientific achievements, and perhaps the most powerful psychological weapon of World War II, was the atomic bomb which was dropped on Hiroshima on August 6, 1945. This and the later one dropped on Nagasaki were primarily responsible for bringing the war to an end, and opened a new era in man's understanding of warfare. As one leading physicist has pointed out, it was not one atomic bomb, or two, which brought surrender, but rather the experience of what one atomic bomb will do, *plus the dread of many more* that was effective.

Atomic scientists have pointed out that nuclear knowledge of this kind cannot forever be kept secret, and that no adequate defenses are known or can be devised. It is also conceded that the use of atom bombs in another war could mean the end of civilization as we know it. Because of the enormity of the implications of these new scientific developments, plans were formulated immediately after the war for the development and control of atomic energy for *peaceful* uses. One of the tests made on the bomb after the war, known as "Operation Crossroads," may well have been significantly named—man's choice of the use of atomic energy for peace or war. On a purely rational basis, the decision would seem clear.

As a psychological factor, the atom bomb had an indisputable effect on the whole world. All the old concepts of war were changed overnight, and a covert fear gripped people of all nations, including those of the United States, which was at that time the only country in possession of atom bombs, or of the "secret" of producing them. The existence of the bomb imposes a great moral obligation on people throughout the world to outlaw its use for war; and the means of its ultimate control rests with a strong international organization.

This is certainly the view of most physicists and atomic scientists, and it was not long after the war before organizations were formed such as the Emergency Committee of Atomic Scientists, and the Association of Education for Atomic Energy.

The present book by two social scientists is based on a report prepared for the Committee on Social and Economic Aspects of Atomic Energy of the Social Science Research Council. It is primarily a summary of a study made in 1946 of American attitudes regarding foreign affairs, especially in relation to atomic energy.

The most interesting thing psychologically is not simply the vast ignorance of large segments of our population regarding world affairs—for example, one-third of the people interviewed could not give even the simplest answer to the question, "What is the main thing the United Nations organization is set up to do?"—but rather the contradictions in attitudes. For example, the majority of people interviewed indicated that they approved the idea of going beyond the idea of a United Nations to a world government, but at the same time objected to lowering tariff barriers. Likewise, most people were veering away from the notion of isolationism, but at the same time did not think it wise to give or lend money to people of other nations. Of course, contradictory attitudes of which these are illustrations are well known to social scientists and others who have carried out attitude studies.

Regarding the atom bomb, most people apparently felt so completely helpless regarding its potential damage that in one sense it had almost ceased to have reality for them. Their attitude was largely one of "let the government worry." In fact, this same attitude of preferring to leave the entire problem up to the "leaders," the "experts," or the "government" has been illustrated in recent studies reported by the Society for the Psychological Study of Social Issues. The main lesson in all of this is the enormous psychological distance between the citizens of our democracy and the world issues that concern their elected and appointed leaders.

The present volume is presented in fairly technical form, including a great deal of tabular material. Apparently it was not written for popular consumption. An index at the end would have been helpful.

STUART HENDERSON BRITT.

*McCann-Erickson, Inc., New York City.*

WOLBERG, L. R. *Medical hypnosis*. New York: Grune & Stratton, 1948. Vol. I, pp. xi+449; Vol. II, pp. vii+513.

These two volumes are concerned with the principles and practice of hypnotherapy. While the title of this work suggests material from the general field of medicine, its contents indicate that the author's main concern is with the field of psychopathology. It may be stated at the outset that Wolberg does not consider hypnosis as the ultimate in therapy, but rather as a useful and legitimate tool, which like any other therapeutic procedure, has its indications and contraindications.

Volume I includes brief but adequate discussions of the traditional topics in hypnosis, i.e., history, phenomena, tests, technics, theories. The greater part of the first volume deals with the direct and adjuvant use of hypnosis in such varied disorders as alcoholism, psychosis, conversion hysteria, anxiety neurosis and psychosomatic conditions. In addition there is a brief statement of the possible use of hypnosis in such miscellaneous conditions as overweight, insomnia, nail biting, stage fright and excessive smoking. Preceding the discussion of therapy in each of the above conditions is an analysis of the personality dynamics involved. Direct and complete recording of patient-doctor conversation in three cases makes up the bulk of the second volume. The shortest of these histories comprises 85 pages. The cases illustrate the use of hypnosis in symptom removal, psychobiologic therapy (guidance, reassurance, desensitization, reeducation) and psychoanalytic therapy, respectively. The use of hypnosis in any but the latter case involves, Wolberg warns, the risk of discharging a patient symptom-free but without a full understanding of his inner conflicts. Justification, therefore, for the use of hypnosis in symptom removal or psychobiologic therapy is to be found only by a careful scrutiny of certain aspects of the personality (ego strength, the role of the symptoms in the total personality, motivation to be symptom free), certain situational factors (money and time available), and the therapeutic goal considered feasible.

The question of therapeutic goals is too often ignored and it is here that Wolberg, by his thorough discussion of this and the related topic of symptom treatment, makes his most pertinent contribution. Also of interest is the author's theory of hypnosis and his observations concerning the relative effectiveness of hypnosis and narcosynthesis. In addition, both volumes contain detailed procedural suggestions relating to technics of trance induction, deepening of the trance, eliciting of hypnotic phenomena and self hypnosis.

The chief criticism of this reviewer concerns the tendency of the author to explain away the discrepant data concerning such problems as changed sensory limen, individual differences in susceptibility, and validity of regression in terms of aptitudes. These aptitudes are said to be determined by the interplay and balance of both conscious and un-



conscious strivings within the individual. The subject who is hypnotized "against his will" or by a disguise technique is thought to be strongly motivated toward susceptibility on the unconscious level, while in the cooperative but nonsusceptible person, the reverse situation would hold. The data supporting this hypothesis, however, are of a post hoc nature and the hypothesis itself is not easily subjected to experimental verification. Another objection is Wolberg's use of Rorschach test results in support of the reality of regression. This would seem to assume the existence of well standardized age norms.

The final chapter on the future of hypnosis is both brief and weak. The author argues that in the interests of both medical and lay groups, safeguards (i.e. laws) are required which would restrict the use of hypnotherapy to members of the medical profession. There are many objections to this conclusion. Not the least important of these is the fact that, as the author's own references indicate, many individuals not privileged to belong to the ranks of the medical profession have made contributions both directly and indirectly to the field of hypnotherapy.

In these two volumes, Wolberg has presented a well written and interesting work which makes a definite contribution to the field of hypnosis. These books may be profitably used as reading material in advanced courses in abnormal and clinical psychology.

F. L. MARCUSE.

*Cornell University.*

HALL, VICTOR E. (ED.). *Annual review of physiology*. Stanford, Calif.: Annual Reviews, Inc., 1948. Pp. xi+552.

For the psychologist not intimately concerned with specific physiological processes, there is relatively little of value in the volume. Of the 23 chapters, only five are oriented toward problems with which the profession commonly deals. These five are: (1) Vision, by A. Chapanis, 24 pages; (2) Physiological Psychology, by W. H. Gantt, 26 pages; (3) Heat and Cold, by D. H. K. Lee, 22 pages; (4) Anoxia in Aviation, by L. F. Nims, 10 pages; (5) Somatic Functions of the Central Nervous System, by W. S. McCulloch and J. Y. Lettvin, 16 pages. The remaining chapters contain isolated, relevant studies for the psychologist, but they are scarce.

Gantt's contribution on physiological psychology is frankly limited by the author to the consideration of his own field of interest, Pavlovian methodology and human psychopathology. However, for those similarly occupied, the references to current Russian experimentation and the expostulation of the need for a "level of integration" approach to psychobiology are rewarding. Psychologists need the fertilization which comes from such considerations of integrative levels as are presented in Novikov's paper in *Science* (March 2, 1945, 101, 209-215). The reviewer found Chapanis' chapter a concise and worthwhile

### BOOK REVIEWS

survey of the salient new studies on color vision. Considering the mass of material emerging from the war, it is an achievement to have isolated the significant studies as Chapanis has done.

McCulloch's review is concerned mainly with the research done in his laboratory and related areas. Most of the references cited at the end of the chapter are not described in the text. For those dealing with the physiological substrata of oxygen deprivation, Nims' brief summary will be of particular interest. It would have been valuable to have related the high altitude effects of anoxia with those reported for such psychosomatic therapies as insulin and electroshock. Lee's contribution is an effective presentation of the environmental pressures arising from exposure to heat and cold. Many of the data offered are rarely cited in psychological literature.

It is to be hoped that the editors of the forthcoming first "Annual Review of Psychology" will follow the example of the editors of this volume in their attempt to ensure author representation from all parts of the world regardless of "curtain effects."

BERNARD F. RIESS.

*Hunter College.*

MOORE, THOMAS VERNER. *The driving forces of human nature and their adjustment: an introduction to the psychology and psychopathology of emotional behavior and volitional control.* New York: Grune & Stratton, 1948. Pp. v+456. \$6.50.

The reviewer hazards the guess that the late Father Moore of the Catholic University of America was the sole contemporary psychologist who was not only a Ph.D. and an M.D., but also (as the title-page declares) a "monk of the order of St. Benedict." Since stereotypes victimize those who "know about" them as well as those ignorant of the concept, some professional colleagues may be wrongly disposed to view this volume as just another re-statement of Thomistic doctrine. Actually, it is an original and powerful synthesis of experimental psychology and clinical psychiatry with "the Perennial Philosophy."

There are seven parts to this treatise which is pitched on an intermediate or advanced level:

The *first* is a brief personalized history of the foci of American psychology with one of the most penetrating treatments in print of the deficiencies in the logic and philosophy employed by William James. Moore's own agreeable modernity is revealed in his evident distaste for "the Latin terminology of certain neoscholastics" and his insistence that "It is of importance in the study of the human mind to turn to the mind itself rather than to the commentaries on Aristotle and St. Thomas" (p. 44).

The *second* part, on the Unconscious, closes with a brilliant discussion of the components of mind wherein Moore champions a tripartite division into functions, products, and dispositions. Pedagogically, his classificatory table of mental "elements" on p. 106 is one of the best of its kind.

Part II on the Emotional Life centers about a thorough demolition of the Lange-James theory. The author holds that "affections" are as much psychic as organic and that "the human being is a psychosomatic unit substance." "Faculties" are accepted as real entities on two grounds: (1) the necessary existence of mental "functions" to account for "products," and (2) the findings of factor analysis pointing to independent or distinguishable mental processes.

The *fourth* part is a rich treatment of representative psychopathological cases grouped under conventional categories. Here Moore's vast and solid learning, as evidenced by the extensive French, German, and Spanish footnotes, and his broad experience in "helping people in trouble" are prominent features.

The heart of this work appears in Part V which bears the same title as the book itself. It outlines a system of motivation with these emphases: "There are just as many impulses as there are human abilities"; "the criterion of worth in evaluating desires is not pleasure but accomplishment"; self-management requires a hierarchy of desires with one supreme end of life to which everything else must conform; the self-ideal is an ideo-motor concept; "religion is the natural sublimation of human desires, always possible and always effective, no matter how great the calamities that confront us"; man has many drives and not one. The twin chapters on the "parataxes" of depression and anxiety in this section are little works of art.

In Part VI the "Will" (with a capital W) is rehabilitated by arguing that if there is an admitted function of control, there must be a volitional faculty, which in turn is essentially a part of the "soul," or total organizing principle of man. Actually, this theory, while stressed, receives less space than extended analyses of experiments on motor response wherein Moore is thoroughly at home.

The *seventh* and concluding part is devoted to mental hygiene principles and a related philosophy of nature and society. There theology seems to win the upper hand, for the tone is basically that of a psychology of religion from the believer's standpoint.

Few books are harder to challenge than this one since it seems to be unusually free of factual errors. Certain judgments, of course, may be questioned, such as his belief that cyclothymia is transmitted in the genes, or the opinion that most of the "whining evaders of responsibility" he encountered in hospital wards had grown up without altruistic urges. One may also wonder whether there is not something spurious or at least weak in the (metaphysical ?) presuppositions behind his clever caricature of the literal superficiality of Wundtianism (or any other form of psychological positivism), viz: "There is a flow of consciousness, but what flows? Nothing. There is sensation and perception, but what perceives? Nothing. There is choice, but who chooses? Nobody. There is action, but nothing acts; motions and nothing moves" (p. 326). The field conception of the self-distribution of forces appears at the very end in a short but somewhat unintegrated discussion of the growth process, with the treatment implying that this position is all right as far as it goes, but that it doesn't go far enough. An astounding

omission, too, in an earnest scientific testament of exceptional scope is the absence of any consideration of the cultural anthropologist's approach to personality structure.

Any one who finishes his perusal with the experience of "almost thou persuadest me" must acknowledge that this is a serious and important contribution to systematic psychology, of potential benefit to a wider audience than it will probably have.

GEORGE W. HARTMANN.

*Teachers College, Columbia University.*

POFFENBERGER, A. T. (ED.) *James McKeen Cattell: man of science. 1860—1944.* Lancaster, Pa.: Science Press, 1947. Vol. I, pp. viii + 582; Vol. II, pp. viii + 503.

Professor Poffenberger has rendered an important service to science and has paid tribute to a great scientist, teacher and apostle of academic freedom by bringing together in convenient form seventy of the more outstanding papers of James McKeen Cattell (1860—1944). The first volume contains twenty-nine of his scientific papers in which original researches are reported. In the second volume are forty-one papers, mostly addresses and semi-popular articles. Each volume has a short foreword by the editor followed by an appreciation of Cattell, in Volume I by R. S. Woodworth and in Volume II by F. L. Wells.

Cattell's complete bibliography, from which these seventy papers were selected, contains 167 titles which are listed in the back of Volume I. Of these 30 or more are reports of research work and constitute the bulk of Volume I. Those selected for the contents of Volume II are largely addresses, book reviews, notes and comments, most of which appeared in *Science* or *The Scientific Monthly*. Only two books are listed in the bibliography—*University Control*, published in 1913, and *Carnegie Pensions*, in 1919. Cattell believed that the best way to disseminate scientific information is not through books but by short papers in scientific and popular journals.

The editor does not state the basis on which the 41 papers for Volume II were selected. It appears, however, that they are representative of Cattell's broad interests in the advancement of science, its usefulness in daily life, and in his deep concern for academic freedom and democracy. More than half of the papers reprinted in Volume II are on psychology and its applications; about one quarter are on topics concerning the advancement of science in general; and one quarter on the organization and administration of colleges and universities in the United States.

In each volume the papers are arranged in chronological order and with no editorial change or comment. In view of the assertion by the editor that these papers have more than a mere historical value, one wonders why a topical arrangement was not employed. With the ex-



ception of two short papers (and possibly one other) the contents of Volume I could have been arranged under four main topics representative of the four major areas in which Cattell did his important research. These are:

1. *Studies of reaction times*, including measurements of the time taken up by the inertia of receptors, by sensation, perception, association, recognition, etc. At least ten of the papers in Volume I are reports of experiments in this general problem area.
2. *Studies in psychophysics*, particularly his famous work on the perception of small differences and measurement of errors of observation. The longest paper in Volume I—more than 100 pages—is his famous study, "On the perception of small differences," done in collaboration with Professor G. S. Fullerton of the University of Pennsylvania and published in 1892.
3. *The development of mental tests* and the measurement of individual differences. There are two papers on this topic in Volume I and three in Volume II.
4. *The development of the order of merit method* and its applications in his famous work on men of science. Ten papers comprising most of the last 200 pages of Volume I are on this general topic.

The papers in Volume II could have been classified as follows:

1. Those in which Cattell discusses technical problems in psychology, as for example, the papers on "The time it takes to think," "Reactions and perceptions," "The perception of light" and "Mental measurement."
2. Those in which he states his general viewpoint in psychology and his conceptions of psychological problems. For example, "The conceptions and methods of psychology."
3. Papers on the applications of psychology to practical problems, as for example, "Practical psychology," "The usefulness of psychology," "Psychological methods to promote highway safety."
4. Papers on the organization of scientists and the advancement of science.
5. His extensive studies on the organization and control of American colleges and universities, including two or three papers on general education in relation to science and democracy.
6. And finally, two or three papers on wider social issues—democracy and international good will.

This outline will give the reader a general idea of the contents of these volumes. Psychologists will find in Volume I not only the original data of Cattell's early experiments but very stimulating discussions of problems and methods that are quite relevant to modern experimental work. This volume should and undoubtedly will appear on the reading lists for graduate students of psychology. It is a classic that will brighten with age. Cattell did as much as any contemporary psychologist to put psychology on a sound scientific basis and far more than any other man to establish it as a respected member of the growing family of sciences. He was the first man in the world to hold the title "Professor of Psychology" and the first psychologist to be admitted to the National Academy of Sciences.

Although Cattell never attempted to found a school of psychology



or to formulate and promote a conceptual system, he none the less held rigidly to a behavioral point of view and regarded psychology as a growing and important science of behavior. "I have always held that psychology has to do with the conduct of an individual rather than with his consciousness. We are concerned with what a person does, rather than with what he thinks he thinks, feels he feels or imagines he imagines. In psychological experiments, as I once put it, 'It is usually no more necessary for the subject to be a psychologist than it is for the vivisected frog to be a physiologist'" (II, p. 377).

Although Cattell had little use personally for the introspective studies of consciousness and mental states, yet he maintained a healthy respect for all honest scientific efforts. He believed that psychology would grow as a science by the application of scientific methods, in the spirit of science, to its broad range of problems. In several places in his writings there appear statements like this: "The two great achievements of science have been the elaboration of the quantitative method on the one hand and of the genetic method on the other. . . . The physical sciences have been primarily quantitative and the biological sciences are primarily genetic . . . Psychology is from the start both quantitative and genetic" (II, pp. 203, 205). By the genetic method he meant the study of the previous conditions on which measurable activity depends. "Every mental state and every form of activity is the result of development from previous conditions. If explanation, as distinguished from description, is possible anywhere in science it is possible here" (II, p. 205).

Cattell never engaged in acrimonious controversies with other psychologists over points of view or scientific issues. His fighting talents, of which he had many, were used in other directions. They were aimed mainly at some university presidents, at boards of trustees, and particularly against the autocratic system of university control. The gallant fight that he led for academic freedom was prompted by his devotion to the three greatest things of life—science, democracy, and education; the greatest of which is science. These constitute "the triangular base on which rests the civilization that we have." They cannot be divorced. They must stand or fall together. They are centered in colleges and universities which are the sources of the greatest scientific achievements of our times. Universities must therefore, above all other institutions, be democratic in spirit and control. Cattell believed that one of the greatest stumbling blocks in the road of scientific and democratic progress, and hence in the path of human welfare and happiness, is the autocratic control of colleges and universities.

He tackled the problem in the democratic fighting spirit of Andrew Jackson. He loaded his gun with relevant facts charged with biting humor and fired them mainly at university presidents and boards of trustees. In his book on *University Control* he said, "In the academic jungle the president is my black beast . . . I once incited one of my

children to call her doll Mr. President, on the esoteric ground that he would lie in any position in which he was placed. Of course, the president is by nature as truthful, honorable and kind as the rest of us, and is as likely to have more ability or enterprise, or both. But he really finds himself in an impossible situation" (II, p. 284). He goes on to propose certain reforms in university control which in his judgment would clear the way for the progress of science and the welfare of humanity.

The subtitle of the two volumes is "Man of Science." Cattell was all of this and much more. He was a great protagonist of science. Science to him was the most vital thing in life, amounting almost to a religion. Scattered through his writings, even in his most technical papers, are numerous references to the great importance of science for civilization and human happiness. It is the basis of democratic society; the discovery of truth that sets men free. It has freed them from toil and drudgery, from human slavery and child labor, from physical disease and pain, from torturing fears, and from "jealousies, resentments, and passions that lead to war." "Science has not only created our civilization: it has given to it the finest art and the truest faith" (II, p. 406).

Cattell was not a science-for-science's-sake man. For him science was not an indoor sport to be enjoyed by a chosen few. He regarded it as a powerful instrument of human welfare. His interest in applied psychology and his organization of The Psychological Corporation, to which he devoted the last years of his active life, bear eloquent testimony to this conviction. A characteristic statement of this belief is, "... it appears to me that the conditions are most healthful when science and its applications proceed hand in hand, as is now the case in engineering, electricity, chemistry, medicine, etc. If I did not believe that psychology affected conduct and could be applied in useful ways, I should regard my occupation as nearer to that of the professional chess-player or sword swallower than to that of the engineer or scientific physician" (II, p. 206).

There is an interesting parallel between the careers of Cattell and Benjamin Franklin. Both were interested in the advancement of science and particularly in its application to the practical problems of life. Both were journalists and devoted to the wide dissemination of knowledge. Each was the founder of a scientific society. Both were socially minded citizens: strong believers in the democratic process and bitter opponents of tyranny and regimentation, particularly in matters of scientific inquiry. There are, of course, important differences in the careers of these two great Americans, one of which may be changed as time goes on. The name of James McKeen Cattell may some day be as well known throughout the world as that of Benjamin Franklin.

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GORER, GEOFFREY. *The American People. A study in national character.* New York: Norton, 1948. Pp. 246.

The scientific approach to the study of national character is beset by three major problems: what do we want to know, how are we going to get it, and how shall we interpret what we get? Cultural anthropologists interested in describing and explaining the general contours of small societies and their cultures have been able to overcome in some measure these problems faced by the scientist in the study of large, often heterogeneous, national groups. For, almost in one bold swoop, it has sometimes been possible for the culturologist to survey the social structure of a small tribe, describe its institutions and how they interlock with one another, make predictions as to how these institutions must inevitably affect the individuals involved as individuals and as a collective, and then to give case histories of a selected sample in order to show that the over-all description, explanation, and prediction square with the case material, i.e., with how the individuals actually think, feel, and act. These studies have for the most part approached the problem with psychoanalytic orientation so that not only are the assembled facts interpreted within this framework, but also the selection of facts demanded by the theory occupy the attention of the researcher, often at the expense of other ones equally important. Nevertheless, though these studies are really not as compact and all-explanatory and all-wise as they often seem when first one reads them, they have contributed greatly to our vision of the relations between the individual and his society, the flexibility and the basic stuff inherent in "human nature," as well as theoretical and methodological insights in the conception of "personality," "culture," "society," "institution," "social change," "normality," and "abnormality."

It is from this perspective that Geoffrey Gorer, like Margaret Mead<sup>1</sup> before him, has attempted to level his anthropological sights at the American people and to unearth their fundamental characteristics in one fell swoop. He was especially interested in making a contribution to the "mutual understanding and fruitful collaboration of the English and American peoples and governments, [because] such understanding and collaboration are our only safeguard against the inconceivable horrors of another war, or the horrors, only slightly less grim, of totalitarian domination" (p. 22). In this attempt, "only about two thirds of the inhabitants of the United States" are studied, thus excluding "the history, the traditions, and even to a large extent the population of the southern states and to a lesser degree Texas, rural New England, and California . . ." (p. 15). He further does not consider "the more obvious minorities, whether ethnic, religious, or social," within the confines of his vaguely delineated boundary-lines. Because of these considera-

<sup>1</sup> MEAD, MARGARET. *And keep your powder dry.* New York: Morrow, 1942.

tions, one gets the feeling from the heart that not only are forty million Americans neglected in the study but that it is a study tailored-to-fit the "Anglo-Saxon" section whence the basic character of America is *supposedly* derived. This, of course, is a supposition which is taken altogether too much for granted by Gorer and can easily stand further investigation.

The study is largely impressionistic and based on Gorer's travels throughout the United States as well as his discussions with various social scientists and his "love and friendships and quarrels and misunderstandings and delicate negotiations and casual incidents which made up my life in the United States" during a period of seven years. Among a host of unrelated, though interesting, impressions and facts thus gained, Gorer presents a simple thesis which is supposed to "explain" what gives Americans their national character.

The essence of the thesis is not new; it is merely put in different words than others have done, especially Margaret Mead, an intellectual debt which he carefully acknowledges. In brief, we are told that the fundamental characteristic of Americans is their hatred of authority, and that this hatred stems from the fact that America, like no other country,<sup>2</sup> is a haven for immigrants, or at least has been until comparatively recently. First generation immigrants are depicted as having a strong hatred for their respective oppressors and having great hopes of adapting as best as possible to the new freedom of America, especially in the upbringing of the second generation as more perfect Americans than they are, in language, manners, appearance, aims, and values. "It is this break of continuity between the immigrants of the first generation and their children of the second generation which is to my mind of major importance in the development of the modern American character, which gave rise to what might be called by analogy with genetics, the American mutation. . . . The individual rejection of the European father as a model and a moral authority . . . was given significance and emphasis by its similarity to the rejection of England by which America became an independent nation" (pp. 26-27).

There can be no doubt that there is a great deal of truth in this theory of "Europe and the rejected father" as a basis for the national character of America, but when it is made to answer for almost every characteristic, it falls far short of the mark, as should be expected when one or two explanations are given for a set of infinitely complex and pluralistic social phenomena. When, for instance, we are told that it was only "the less assimilated and less assured groups" who loved Roosevelt like a father, and that the remainder had an "almost pathological hatred" for his authority, then it would seem that Dr. Gorer is overdramatizing the situation in order to fit it to his theory. It seems to this

<sup>2</sup> Modern Palestine might be an exception to this statement, and to a lesser degree, Canada.



reviewer that an economist might more readily explain, and rightly so, the hatred for Roosevelt in terms of his position vis-à-vis the vested interests, and the love for him by the vast mass of the population precisely in terms of his authority in dealing fearlessly with these vested interests.

The crucial test of this theory is perhaps in the sphere of religion. While religious matters do not occupy the population nearly as much as in other countries, they are nevertheless, for the most part, God-fearing people endeavouring to do what is "right in the eyes of the Lord." "In God We Trust" is a common expression in the U. S., symbolizing that the American creed of "Life, Liberty, and the pursuit of Happiness" shall be guaranteed to all *under God* and *under those* whose position it is to administer this *maximum bonum*. Indeed, hatred of authority is manifested everywhere in the U. S., but it is in regard to interpersonal relations and not so much to all positions of authority in the land. It is perhaps because Gorer was not in a position to treat the religious question at all, as he points out in his Foreword, that he missed the significance of the "hatred of authority." It is for this reason that Gorer's Americans seem like a grumbling lot of authority-haters continually unable to distinguish between order and anarchy, between official leadership and self-appointed demagoguery, between duly constituted law and morals and the passing fancy of their individual tastes.

As the complement to the hatred of authority, i.e., the rejected father in psychoanalytic symbolism, is the inordinate love for the mother and things feminine. "The idiosyncratic feature of the American conscience [i.e., the superego] is that it is predominantly feminine," derived from the close ties between mother and child up to the age of six, and thenceforth reinforced by "school teachers who are overwhelmingly women." Business, politics, and the world of things (not persons) is exempt from this morality. For this reason of ambivalence, according to Gorer, "businessmen and their spokesmen" were violently opposed to the New Deal because "its social legislation was felt to be introducing into the domain of masculine privilege the meddling of female morality." Once again, one wonders what an economist or historian would say of such an explanation in their efforts to portray the Great Depression and its consequences. Accordingly, it would seem best for social scientists in the field of national character always to restrain themselves from giving a psychoanalytic explanation for large social issues, and only when no other explanation is forthcoming to feel free, though again with restraint, to resort to psychoanalytic or other psychological concepts. It is with such explanations in mind that Durkheim was prompted to write, and rightly so, many years ago: "Every time that a social phenomenon is directly explained by a psychological phenomenon, we may be sure that the explanation is false." Notwithstanding the corroboration from psychiatrists and other writers cited



by Gorer, the theory of "mommism" may have its place in the consulting room or the clinic, but cannot, without being ridiculous, explain everything from the nursery and the school to the business office and the White House.

A similar criticism could be pointed out when Gorer claims that the "frequently expressed fears that America will be reduced to want, perhaps to actual starvation, if it lets its foods or resources or money outside the country," are due to the predetermined schedule-feeding of infants rather than on the basis of self-demand. Even if the "cause" for such occasional outbursts could be traced to the anxieties created by withholding the breast from the infant, it certainly does not jibe with the well-known fact of America's generosity to less-fortunate peoples, e.g., the aid given after the Japanese earthquake of 1923, the continuous sending of food parcels and money by all sections of the population to relatives overseas, and more recently the vast European Recovery Plan. "The great addiction of most American men to milk as a drink" as well as the "great erotic fetishistic value given to women's breasts" are also taken to be due to this early "anxiety." What is overlooked is that not only do they drink much milk but eat and drink many other things in greater quantities than elsewhere *because the standard of living is higher*; and further, with respect to the other claim, it has not yet been proven that European men are to any degree less erotically excited by women's breasts than American men.

On the question of "ideology" in the American character, Gorer does not have much to say, except to point out that it is "a minor component." While this assertion may be quite true, it seems likewise probable that it is true for the national character of most other peoples.<sup>3</sup> On the other hand, Northrup, Myrdal, and others have found a great deal of ideologism in the American character, so that to say that "ideology is a minor component of Americanism" is at best a controversial contention. Furthermore, some studies in the Psychological Barometer series would tend to show that Americans have pretty strong ideas (whether right or wrong is beside the point) regarding Americanism which guide their conduct, at least to some extent.<sup>4</sup>

Perhaps the most important contribution of the book is its novel analysis in bipolar terms of certain typical relationships: child to father, child to mother, parents to children, husband and wife, and so on. Though this type of analysis was first suggested by Gregory Bateson,<sup>5</sup> it is the first time as far as this reviewer is aware, that it has been as extensively applied in a study of American national character. In the

<sup>3</sup> Possible exceptions might be found among the French, Russians, and Israeli.

<sup>4</sup> Cf. for example, Henry C. Link, What does Americanism mean to the American people? *J. appl. Psychol.*, 1947, 31, 425-430.

<sup>5</sup> BATESON, GREGORY. Morale and national character. In G. Watson (Ed.), *Civilian morale*, New York: Raynal & Hitchcock, 1942.

thematic analysis of the various relationships he has chosen to study, Gorer gives us both an excellent structural and dynamic picture. Americans step out of print and somehow seem more real than they do in the rest of the book. It is a pity this analysis was not extended further.

In fine, notwithstanding the many criticisms that could be levelled at the book, Gorer deserves the thanks of all those who are interested in finding out what makes one people be what they are as compared to another people. Anyone who has thought and worked on this problem is well-ware of the difficulties involved in making generalizations which could easily be accepted by his co-workers. Like Newton's third law, to every generalization in national character there seems to be an equal and opposite generalization. This virgin field is wide-open for research and anyone who seriously attempts to close the wide gaps existing in our knowledge is making a big contribution. But a note of warning seems in place: analysis in depth (e.g., psychological interviewing, description, and analysis) can serve a very useful purpose only if it is then related to an analysis in breadth (e.g., macroanalytic analysis of institutions and social structure as well as systematic polling and attitude studies) and if both are seen in the light of historical and contemporary social, political, and economic dynamics. Each approach separately tells only part of the story. Only an interlocked aggregate of the separate parts can give us the correct picture. This is a hard order but, in a world suffering greatly from international misunderstanding, the results would be worth the effort.

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